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All communications should be addressed to the Editor.

No. 132. DECEMBER, 1921. VOL. XXV.

Notices of the Royal Aëronautical Society.

Election of Members.

The following Members were elected by the Council during the month:—

Fellow.—Major D. H. Kennedy.

Associate Fellows.—H. D. Boultbee, B. W. O. Townshend, H. M. Yeatman.

Technical Discussions.

It has been decided to hold from time to time technical meetings in the Society's Library for the purpose of discussing scientific papers of a more intricate character than those which are suitable for the Society's lecture programme. Any papers submitted will first be considered by the Library and Publications Committee, when, if approved, a date will be fixed for their discussion. A limited number of advance proofs will be prepared and distributed only to members who apply for them, or invited guests. The meetings will consist of discussion only on the assumption that the paper has been previously read by all present. The papers may subsequently be published in the Journal together with the discussions thereon, each individual forwarding his contribution to the Secretary in writing for this purpose.

Memorial Lectures.

In addition to the Wilbur Wright Memorial Lecture, which was constituted in 1912 and for which a capital sum exists in the hands of Trustees sufficient to pay an annual fee of £50, a fund has lately been raised by private subscription to provide a small fee for an annual lecture in memory of the late Wing Commander Nevill Usborne, who played so important a part in the early development of British airships. It is proposed to hold the first Usborne Memorial Lecture in 1922.

Students' Discussion Meetings.

The Monthly Discussion Meetings held in the Society's Library are meeting with welcome support from Student Members. At the second meeting on Thursday, November 10th, Mr. W. L. Le Page read a paper on "The Soaring

Flight Problem," when Mr. F. Handley Page, Fellow, took the chair. These meetings are confined to student members of the Society and their friends, and at the end of the Session the Pilcher Memorial Prize will be awarded to the paper inaugurating discussion at one of these meetings which is judged by the Council to be the best.

R.38 Memorial Research Fund.

The following further donations to this Fund have been received since the publication of the last list in the November issue, bringing the total up to and including November 14th to £1,093 6s. 6d.

	£	s.	d.
Lord Invernairn	105	0	0
W. H. Coats, Esq.	105	0	0
Viscount Cowdray	52	10	0
Viscount Burnham (Proprietors of "Daily Telegraph") ...	25	0	0
Sir John Hunter	20	0	0
Sir Archibald Denny, Bart.	15	15	0
Sir H. McGowan	10	10	0
G. Holt-Thomas, Esq.	10	10	0
Sir Joseph Maclay	10	10	0
Lord Foley	10	0	0
Lieutenant-Colonel J. Dunville	10	0	0
H. E. Yarrow, Esq.	10	0	0
Lord Glentanar	10	0	0
J. E. Hodgson, Esq.	5	5	0
R. P. Wilson, Esq.	5	5	0
Flight-Lieutenant R. S. Booth	5	0	0
Lord Montagu of Beaulieu	3	3	0
Major-General Sir Sefton Brancker	3	3	0
Anon.	3	0	0
A. H. Ashbolt, Esq.	2	2	0
A. P. Cole, Esq.	2	2	0
H. Wyn Evans, Esq.	2	2	0
Rev. Basil Phillips	1	1	0
E. H. Lewitt, Esq.	1	1	0
John Newton, Esq.	1	1	0
S. E. Taylor, Esq.	1	0	0
Flight-Lieutenant John Barron	1	0	0
Captain A. E. Heatley	0	10	0
Major G. H. Abell (Second donation)	0	10	6
Captain J. B. Walker	0	5	0
	£432	5	6

Prizes.

The Council will, at the New Year, consider the award of the Society's Silver Medal to the author of the best paper published in the Journal during the year. The Society's Bronze Medal is similarly open for annual award to the best paper by a Student Member published in the Journal.

Donations.

The Council acknowledge with grateful thanks the gift of the following books from Air Commodore H. R. M. Brooke-Popham on vacating the chair:—"Manual of Meteorology, Part IV.," by Sir Napier Shaw; "The Gas, Petrol and Oil Engine," by D. Clerk and G. A. Burls; "The Heat Treatment of Tool

Steel," by H. Brearley; "Thermodynamics for Engineers," by J. A. Ewing; "Theory of Structures," by Arthur Morley.

Lectures.

The following amended programme of lectures for the remainder of the present Session is published for the information of Members :—

Dec.	1.	"The Present State of Airship Development." Major G. H. Scott.
,,	15.	"Development of the Fighting Aeroplane." Capt. F. M. Green.
1922.		
Jan.	5.	"Specialised Aircraft." Wing Commander W. D. Beatty.
,,	12.	Juvenile Lecture. (To be announced later.)
,,	19.	"Aeroplane Installation." Brig.-Gen. R. K. Bagnall-Wild.
Feb.	2.	"Radiological Research and the Examination of Materials." Dr. V. E. Pullin.
,,	16.	"Methods of Instruction in Aeroplane Flying." Squadron Leader C. F. A. Portal.
Mar.	2.	"Testing Aircraft to Destruction." W. D. Douglas.
,,	30.	"The Design of a Commercial Aeroplane." Capt. G. de Havilland.
Apl.	6.	(Subject to be announced later.) Mons. L. Breguet.

The lectures will be held in the Theatre of the Royal Society of Arts, John Street, Adelphi, at 5.30 p.m.

Arrangements for the Month.

Dec.	1, 5.30 p.m.	Lecture by Major G. H. Scott, C.B.E., A.F.C., on "The Present State of Airship Development," Royal Society of Arts, Adelphi, London.
,,	8, 7.0 p.m.	Students' Discussion Meeting, C. Daniel on "Practical Points in Fuselage Construction," in the Society's Library.
,,	12, 8.0 p.m.	*Scottish Branch.*—Lecture by Colonel Gold, of the Meteorological Dept., Air Ministry, on "The Application of Meteorology to Aviation," Royal Technical College, Glasgow.
,,	15, 5.30 p.m.	Lecture by Captain F. M. Green on "The Development of the Fighting Aeroplane," Royal Society of Arts, Adelphi, London.
,,	16, 4.0 p.m.	Candidates' Committee Meeting.
,,	16, 4.30 p.m.	Publications and Library Committee.
,,	16, 5.0 p.m.	Council.

W. LOCKWOOD MARSH,
Secretary.

PROCEEDINGS.

SECOND MEETING, FIFTY-SEVENTH SESSION.

A meeting of the Society was held in the Hall of the Royal Society of Arts, London, on Thursday, October 20th, 1921, Lord Montagu of Beaulieu presiding.

The CHAIRMAN said that they were gathered there to hear a lecture which he was sure would interest them all. It raised, he understood, some points which might be described as controversial, but he did not think anybody minded controversy if it led to establishing the real truth and put on record what should be the history of the past.

As regarded the lecturer himself, he need hardly introduce Mr. Griffith Brewer. He was one of the oldest of those who had taken an interest in aviation, from the days of ballooning to the various methods of progress through the air to-day. There was nobody more qualified to read a paper on this subject, and he was quite certain that, whatever their personal opinions might be in regard to the controversy, they would all thank him very much for having gone so carefully into the question, which was important on both sides of the Atlantic.

In order that there might be no misapprehension as to the facilities given for reply, he was informed that a full copy of the paper, with the diagrams and photographs, had been forwarded to the following American gentlemen mentioned in the paper:—Dr. Walcott, Mr. Manly, Dr. Zahm and Mr. Glenn Curtiss. They had all been invited to come and take part in the discussion, if they could, or send a written contribution. Replies had been received from Mr. Curtiss and (a cable) from Dr. Walcott. They had not yet arrived, but were in the mail. He wanted to make it clear that the Society had given, and would give, every opportunity for both sides of the question to be heard, and it went without saying that when Dr. Walcott's and Mr. Curtiss's replies were received, together with any others that were sent, they would be printed with Mr. Griffith Brewer's paper in the December number of the AERONAUTICAL JOURNAL.

The chief duty of the Chairman of such a gathering as that was to say as little as possible himself and make way for the lecturer, so he called upon Mr. Griffith Brewer to read his paper.

Mr. GRIFFITH BREWER then delivered the following lecture:—

THE LANGLEY MACHINE AND THE HAMMONDSPORT TRIALS.

In the discussion following my reading of the Wilbur Wright Memorial Lecture, before the Royal Aeronautical Society, on June 6th, 1916, Lord Northcliffe drew attention to the attempt which had been made to rob the Wright Brothers of the credit of inventing the aeroplane.

> "We have not heard much of that in England," said Lord Northcliffe, "but 'a prophet is not without honour save in his own country,' and in the United States there have been long and persistent attempts to belittle the work of Wilbur and Orville Wright. I have closely read and followed the history of the hundred years of aeroplane experiments, and I am convinced that the credit of the first flying machine is due to the Wright Brothers, and from the point of practical flying to nobody else. As an Englishman I am in an independent position, and I know that these words of mine will

go across the Atlantic, and I believe they will assist in stopping the spread of the insidious suggestion that the Wrights did not invent the aeroplane."

The omission in my Paper, referred to by Lord Northcliffe, was due to the fact that I had not sufficient facts at that time to completely disprove the good faith of the endeavours which were being made to prove that it was Langley and not the Wrights who had brought in the era of practical flying. I had, however, already in America in 1914 commenced some investigations into these attempts to distort the history of flying, and had recorded in an article I wrote in the "New York Times" on June 22nd, 1914, my protest against the alterations then being made at Hammondsport to the original Langley machine.

I have recently returned from a further investigation in America, and I am now in a position to report to this Society, not only that Lord Northcliffe's statement is fully justified, but that the Hammondsport trials have been inaccurately reported to the Smithsonian Institution. An official report declaring that the Langley machine had been flown at Hammondsport has since been issued by the Smithsonian Institution.

Since the publication of this Smithsonian report, Langley has been widely accepted as the pioneer of aviation, and Langley is now widely credited with having invented and constructed the first man-carrying aeroplane capable of sustained free flight.

The Smithsonian Institution has always attributed the failure of the Langley machine in 1903 to a failure in the launching apparatus, and it has hitherto been generally accepted that the machine was wrecked without having had a fair opportunity to prove whether it was capable of flight. It is easy, therefore, to understand that since the Wrights, who had been working on the same problem, succeeded where Langley had failed, Langley's friends eagerly welcomed the suggestion made ten years later that it might still be possible to prove that Langley's machine was capable of flight. If Langley's machine could be flown, one of the most dramatic events in the history of aviation would be accomplished! Langley and the Wrights had been working concurrently, hundreds of miles apart, for several years, and both had approached the final stage of their independent experiments at the same time. Langley tested his machine a few weeks before the Wrights were ready to test theirs, and the attempts to fly the Langley machine failed. The Wrights, one week after Langley's second attempt, tested theirs and succeeded. Was it merely a mishap which robbed Langley of the credit of being the first to fly, or did he fail because the machine he had built was not capable of flight? It is not surprising, therefore, that when Mr. Glenn Curtiss offered to fly the old Langley machine, Secretary Walcott, who had succeeded Professor Langley as Secretary of the Smithsonian Institution, should have welcomed the opportunity to have Langley's machine tested. It cannot be denied, however, that it was unwise to accept Mr. Curtiss' offer to carry out the tests, because the Curtiss Aeroplane Company had just been adjudged an infringer of the Wright Patent, and Mr. Curtiss obviously might have had other motives than merely the vindication of Langley. It certainly was not proper to appoint Dr. Zahm to represent the Smithsonian Institution, when he was known to have been the technical expert witness for the Curtiss Aeroplane Company in its former suits, and when he was about to represent the Curtiss Company in another infringement suit, in which these tests were to play a most important part. This, however, was the situation, and is verified by the fact that although Dr. Zahm says that the Smithsonian Institution agreed to pay the costs of the experiments, none of the costs apart from the carriage of the machine to Hammondsport have ever been paid by the Institution.

The construction of the Langley machine of 1903 is described in the Langley Memoir of Mechanical Flight, published by the Smithsonian Institution in 1911.

A copy of this work has been presented to the Royal Aeronautical Society, so that it may be used for the purpose of verifying the statements contained in this Paper.

Figs. 1 and 3 of the diagrams in this paper, showing certain features of the Langley machine, are from drawings in the Langley Memoir. The construction of the machine used at Hammondsport is shown in the photographs and in Figs. 2 and 4 of the diagrams accompanying this Paper.

Plates 1 and 3 are from the Langley Memoir of Mechanical Flight.

Plates 8, 11 and 13 are from photographs, in Zahm's official report on the Hammondsport trials, published in the Smithsonian Annual Report of 1915.

Plate 9 is from the Secretary's Report in the same publication.

Plate 12 is from the affidavit of Zahm as a witness for the Curtiss Company in the Wright-Curtiss Patent suit.

Plates 2, 4, 5 and 16 are from photographs which I purchased at the Smithsonian Institution in April, 1921.

Plates 6, 7 and 14 are from photographs by Benner, a photographer at Hammondsport, and were secured in 1915.

Plate 15 is from a photograph taken by Lorin Wright on June 4th, 1915.

Plate 10 is from a photograph taken by myself, June, 1914.

The plates reproduce but a few of the many photographs of the Hammondsport machine which I have examined, and which show the alterations mentioned in this Paper.

Figs. 2 and 4 are sketches, made to scale, showing the features of the Hammondsport machine, corresponding to those shown of the Langley machine, illustrated in Figs. 1 and 3. Figs. 2 and 4 were made from the photographs and from information furnished in the affidavit of Zahm in the Wright-Curtiss Patent suit. Most of the changes from the original Langley machine, shown in Figs. 2 and 4, can be verified by an examination of the photographs here reproduced.

In April, 1914, the original Langley machine, which had collapsed on the two attempts to launch it in 1903, was sent by the Smithsonian Institution to the Curtiss Aviation Field, at Hammondsport, N.Y., and entrusted to Mr. Glenn H. Curtiss. A modified machine was built up at Hammondsport, under the direction of Mr. Curtiss, and under the observation of Dr. Zahm, and flights were then attempted to be made with this Hammondsport machine. An official report, written by Dr. A. F. Zahm, was afterwards issued in 1915 by the Smithsonian Institution, proclaiming that flights had been made at Hammondsport, N.Y., in 1914, by the original Langley machine. (See Annual Report of Smithsonian Institution, 1915.)

This Hammondsport machine was afterwards returned to the Smithsonian Institution at Washington, D.C., and the original portions still remaining were utilised in the reconstruction of a facsimile of the original Langley machine. This reconstructed machine is now exhibited in the U.S. National Museum with the following inscription :—

ORIGINAL LANGLEY FLYING MACHINE, 1903.

> The first man-carrying aeroplane in the history of the world capable of sustained free flight. Invented, built, and tested over the Potomac River by Samuel Pierpont Langley in 1903. Successfully flown at Hammondsport, N.Y., June 2, 1914.

I am here to-day to show members of the Royal Aeronautical Society that both the Smithsonian Report and the inscription on the Langley machine are

misleading and untrue. No attempt was made at Hammondsport to fly the original Langley machine. At Hammondsport fundamental aerodynamic alterations were made to the machine which were not mentioned in the report by Dr. Zahm to the Smithsonian Institution. This Hammondsport machine was then put through so-called flying tests, altered after almost every test, and the tests were reported as flights of the original Langley machine. The Smithsonian Institution was led to believe and falsely proclaim that Langley had succeeded, where in fact Langley knew that he had honourably failed.

I fully appreciate the serious responsibility I am undertaking in making these statements, but I am confident that when you have heard the evidence which follows in my paper, which is only a part of the evidence in my possession, you will recognise that an undesirable fiction has been imposed upon the Smithsonian Institution and on an unsuspecting public.

It is of the greatest importance that this Society, in its position as the oldest aeronautical body in the world, shall have its records truthfully maintained. Already Professor Bairstow has, in his Wilbur Wright Lecture in 1919, been misled by the Smithsonian Report into telling this Society that a flight had been made during the war period by the man-carrying aeroplane which Langley designed and made; and as recently as this year Mr. Vivian and Lieutenant-Colonel Lockwood Marsh, in their otherwise excellent "History of Aeronautics," state that it has been proved in these later years that the machine which Langley launched in 1903 was fully capable of sustained flight, and that but for the two accidents, the honour which fell to the Wright Brothers would have been secured by Samuel Pierpont Langley. It is no disparagement of Professor Bairstow, Mr. Vivian, nor of the genial Secretary of the Royal Aeronautical Society, to say that they believed the Smithsonian Report, but it is all-important that they in common with all our other members should learn the truth, and so be saved in future from passing on fiction as truth to those who look to them for facts.

As many members may not be acquainted with the original Langley machine I will briefly describe the main features of its construction and operation, and also the attempts which were made by Langley to fly this machine in 1903.

Photographs, Plates 1 and 2, are a side view and front view respectively of the Langley machine on the houseboat on the Potomac River in 1903, and Fig. 1 is a scale side view diagram of the same Langley machine, showing the more important members and their arrangement.

WW the wings set at an angle of 10 degrees with frames F. As shown in Plate 2, the wings had a dihedral angle to provide lateral balance.

SS the central wing spars.

S^1S^1 the forward wing spars.

C the pilot's car.

M the Langley 5-cylinder radial motor.

P one of the two propellers (the other being exactly behind cannot be seen in the diagram).

K a large fixed keel surface.

V a vane rudder composed of two planes separated at the rear, thus forming a wedge. It is hinged to the keel surface K and made operable from side to side for the purpose of steering to right or to left.

R a large dart-shaped tail, the horizontal surface of which is set at a negative angle of six degrees with the main frame, forming a Penaud tail. This tail R was attached to the frame so that it could be adjusted up and down to different angles, but could not be turned from side to side for steering.

GG single guy posts for bracing each set of wings. The part of each guy post below the main frame F was of wood, having a maximum diameter of

1¼ins. The part above the main frame was of ⅞in. steel tube. From the top and bottom of the guy post G, wires run to the spars S and S^1, the guy post being midway between the two spars. I call particular attention to the location of these guy posts, which was only 28 per cent. back of the front edge of the wing, because I believe it will help to explain the failures which happened on both occasions when attempts were made to fly the machine in 1903.

The Langley machine above described was launched from a houseboat on the Potomac River. The machine was fastened to a car which ran on a track on the top of the boat. Besides the thrust of the propellers, a powerful spring was used to assist in giving the machine the necessary initial speed. In the first test on October 7th, 1903, the machine after leaving the launching car dived, as is shown in Plate 3, and plunged into the water a short distance in front of the houseboat. At the time there was no thought that the launching apparatus had not performed properly, as is indicated by the statements made by Mr. Reed, who observed the launching from the houseboat, and by Mr. Manly, who was in the machine. Mr. Reed stated that—

> "Immediately upon my releasing the machine the launching car which carried it dashed down the track, and it appeared at first that when the car reached the end of its track the machine had been successfully launched." (Reed Affidavit, p. 17),

and Mr. Manly, immediately upon his return to the houseboat after plunging into the water, made the following statement to the Associated Press:—

> "It must be understood that the test to-day was entirely an experiment, and the first of its kind ever made. The experiment was unsuccessful. The balancing, upon which depends the success of a flight, was based upon the tests of the models and proved to be incorrect, but only an actual trial of the full-size machine itself could determine this. My confidence in the future success of the work is unchanged. I can give you no further information. I shall make a formal report to Secretary Langley." (Langley Memoir, p. 266.)

It will be observed that Reed thought "the machine had been successfully launched," and that Manly did not remember at that time anything in the launching to even excite suspicion that the launching apparatus was in any wise at fault.

Plates Nos. 3 and 4 are from photographs taken simultaneously just after the machine had left the launching ways. Plate No. 3 shows the machine diving towards the water, and Plate No. 4 shows a view taken from the houseboat looking at the under side of the machine as it dived. The forward wings, it will be observed, are bent out of shape, the back edges being turned upward until the inner portions of these wings have come in contact with the projecting propeller frames which normally were about 18ins. above. The outer portions of the wings, not being restrained by this frame-work, are bent up still farther, as Plate No. 4 clearly shows. It also will be observed that the rear wings are likewise distorted, though to a less degree.

After the machine had been recovered from the water it was discovered that the lower guy post G, of the front wings, was bent backward, and that the front end of the metal frame-work was bent downward. (Langley Memoir, p. 267.) It was also found that the metal cap, which held the front guy post from rising during the launching, but did not restrain it from slipping out forward, had been stretched out of shape. Since this cap was of thin metal, and had to hold the entire lift of the wings during launching, it is not surprising that it was "stretched out of shape." From the fact that the guy post was bent backward it was assumed that in the launching this metal cap had failed to allow the free movement forward of the guy post as it had been designed to do. Some years

later, in describing this launching, Mr. Manly, in explaining that the launching apparatus had failed to properly release the machine, states that he felt a shock when the machine reached the end of the track. But as he made no mention of it at the time it may be fair to assume that when some years later he thought he remembered a shock, what he really remembered was the idea formed after he had learned of the bending of the post. It seems reasonable that his memory immediately after the flight would be more reliable than it was after a lapse of several years.

On December 8th, 1903, after repairs had been made, a second attempt was made to launch the machine.

> "After starting up the engine and bringing it to full speed, the writer (Manly) gave the signal for the machine to be released, and it started quietly, but at a rapidly accelerated pace, down the launching track. Exactly what happened, either just before or just as the aeroplane reached the end of the track, it has been impossible to determine, as all the workmen and visitors had gone to their stations on the various auxiliary boats, except the two workmen (Reed and McDonald) who had been retained on top of the boat to assist in the launching. It had grown so dark that the cameras of Mr. Smillie, the official photographer, were unable to get any impression when he used them, owing to the extreme rapidity of the shutters with which they were equipped. Fortunately, one photograph of the machine while still in the air was secured, which shows the result of what had occurred in the launching and before any further damage had been caused by its coming down into the water, but the all-important question as to just what caused the accident which did occur remains to a certain extent a mystery." (Langley Memoir, p. 271.)

I here show in Plate No. 5 the photograph just referred to. The upper side of the forward wings of the machine is seen at the top of the photograph and the crumpled rear wings below. It will be noticed that the spars of the right forward wing are broken and that the front end of the metal frame-work is again bent down just as it was in the former trial. There has never been any claim that this forward part caught in the launching ways in this attempt. Mr. Manly, who had these wings in front of him during the launching, did not observe that they were being broken by any entanglement during the launching. The rear wings and the Penaud tail broke during the launching.

> "Mr. Reed, the foreman, who (according to Manly) was qualified to observe accurately, not only through his having worked continuously for many years on the machines, but also from his having witnessed the numerous tests of the models, states that from his position near the rear end of the launching track he noticed that at a point about ten feet before the machine reached the end of the track the Penaud tail seemed to have dropped at the rear end in some inexplicable way so that it was dragging against the cross-pieces of the track, and that at the next instant, when the car reached the end of the track, he saw the machine continue onward, but the rudder and whole rear portion of the frame and the wings seemed to be dragging on the launching car." (Langley Memoir, p. 272.)

On this second attempt to launch the machine, no contention has been made that it was the launching apparatus which failed, but it is conceded that the trouble commenced with the failure of a portion of the machine, namely, the falling of the Penaud tail, which apparently was not supported strongly enough to withstand the downward pressure resulting from the fact that it was mounted on the machine at a negative angle. It will be noticed in the photographs and the diagrams that this tail rudder was set at a negative angle.

That the launching apparatus up to this time had been reliable is shown

by Mr. Langley's own statement in his annual report to the Smithsonian for the year 1904:—

> "A new launching apparatus following the general plan of the former overhead one, but with the track underneath it, was built for the models, and it was used most successfully in these experiments, more than a dozen flights in succession being made with it, while in every case it worked without delay or accident. As soon as these tests with the models on this underneath launching apparatus were completed, that for the large machine was built as an exact duplicate, except for the enlargement, and with some natural confidence that what had worked so perfectly on a small scale would work fairly on a large one . . . . it was felt that if this apparatus were exactly similar to the smaller one, it would be the one appliance least likely to mar the experiments." (pp. 115 and 119.)

It is not reasonable to attribute the collapse of the wings to a failure of the launching mechanism, a mechanism which Mr. Langley states had been used most successfully in more than a dozen launchings of models, when there were several perfectly good reasons why the wings would collapse under any conditions of launching.

The wings of the Langley machine had but a slight factor of safety as was shown in the sand tests of the wings as described on page 203 of the Langley Memoir. When a sand load of only 20 per cent. above the flying stress was imposed, most of the ribs were bent from 12 to 13 inches out of shape. Langley could not make the wings stronger, because he considered the machine already as heavy as it could be, if free flight was to be attained. And it is clearly impossible to launch such a machine using an outside force to give it initial momentum without subjecting it to greater stresses than it would be subjected to in normal flight. On the other hand the machine could not carry its own launching device because this slight extra weight would prevent it from flying at all. It should be remembered that the machine had been designed and tested with a view of standing the stresses of flight only. Being rigidly held down to the carriage in launching, it was forcibly prevented from rising into the air when it had attained the necessary speed for flight. But the stress on the wings of the machine so bound down increased as the square of the speed of the machine with reference to the air, and the wings therefore may have been subjected to excessive pressures before the machine was released at the end of the track.

Earlier in this Paper I called particular attention to the position of the guy post G (Fig. 1), which is 28 per cent. from the front edge of the wings. According to the Jöessel formula, the formula generally used at the time to calculate the centre of pressure on an aeroplane surface, the centre of pressure at 10 degrees incidence was a little over 25 per cent. back from the front edge of a plane. Mr. Langley had made no measurements to locate the centre of pressure at small angles. In this case it will be noticed that Langley placed the guy posts just about where any engineer would have placed them if the centre of pressure were believed to be 25 per cent. from the front edge of the wings. But the centre of pressure with change of angle on a curved surface travels at all flying angles in the opposite direction from that on a plane. Langley's machine was designed without taking this fact into consideration. This vital law was first published to the world in the Paper read by Wilbur Wright before the Western Society of Engineers on September 18th, 1901, and it appears that Langley did not profit by this information. Otherwise he would not have placed the guy posts so far forward as in the position shown in the diagram in Fig. 1.

The centre of pressure on the Langley wings would have been about 37 per cent. back from the front edge while being launched at 10 degrees angle of incidence. But at smaller angles of incidence, such as it would have taken im-

mediately upon being released, the centre of pressure would have moved still farther back. The turning-up of the rear edges of the wings and the bending-back of the guy post are but the natural consequence of placing the trussing too far forward of the centre of pressure and of the trussing not being strong enough to withstand this unguarded stress. The breakage of the wings in both tests in 1903 was due to faulty design, and not to a failure of a part of the launching mechanism to perform as it was designed to perform.

I think I have said sufficient here to show the technical members of the Society that the failure of the original Langley machine was due to the collapse of the wings owing to their frailty and to bad engineering in placing the guy posts so far forward of the centre of pressure. The fact that the wings were subject to much greater stress during the time the machine was held down to the launching car, than it would have experienced if it had been free to rise, is surely sufficient reason for the failure of the machine, without assuming the additional accident of the guy posts catching in the launching apparatus. I will not, therefore, labour this point, especially as I am now going to show, when comparing the Langley machine with the Hammondsport machine, that Dr. Zahm and Mr. Curtiss apparently knew of these fatal mistakes in the design of the Langley machine, and attempted to correct them when building the Hammondsport machine.

After the original Langley machine had arrived at Hammondsport, in April, 1914, Dr. Zahm and Mr. Curtiss proceeded to build up the Hammondsport machine partly from the original Langley machine and partly from new parts. (Zahm Affidavit, p. 4.)

Among the many changes existing in the Hammondsport machine at the time of its early trials in May and June, 1914, I will briefly mention the following:—

1. New wings were constructed at Hammondsport in which all of that portion in front of the forward spar of the Langley machine (see Fig. 1 of the diagram) was entirely omitted, and thus the forward spar S^1 formed the leading edge of the Hammondsport wings (see Fig. 2 of diagram and Plates 8, 9 and 14). The camber of the Langley wings was 1 in 12, while that of the Hammondsport machine was about 1 in 18. The area of the Langley wings was 1,040 sq. ft., that of the Hammondsport wings about 990 sq. ft. The Hammondsport wings also had a slightly different aspect ratio from that of the Langley machine. These changes, as is well-known to all aeronautic engineers, produce a wing of entirely different aerodynamic properties from the original Langley wing.

2. In the original Langley machine the wings were stayed by wires to single guy posts GG, projecting above and below the central frame F. The part of the post above the central frame was replaced by two very much larger posts, having about twice the height of the original guy posts and forming an inverted V over the central frame. The single guy post below the central frame in the Langley machine was replaced by four much heavier posts. (See Plates 6, 7, 8, 9, 10, 11, 12, 13, 14 and 15.)

3. The guy wires at Hammondsport were attached to the spars at different points than in the Langley machine, the advisability of which change was made apparent by the breaks in the spars in 1903. (Compare Figs. 3 and 4.) These changes can be seen in some of the original photographs, but are not so clear in the plates.

4. This whole system of trussing, which replaced the guy posts used by Langley, was moved 30ins. farther back, so as to be near the normal centre of pressure on the wings (Figs. 1 and 2, and Plates 7, 8, 9, 12 and 14). I have already called attention to the seriousness of Langley's lack of knowledge in placing these guy posts so far forward.

5. The middle spar of each front wing was strengthened by an additional spar E underneath the ribs and extending outward for about two-thirds of their length.

These middle spars, not reinforced, had collapsed in the tests of the Langley machine in 1903. (Fig. 4.)

6. The wings were further strengthened by the under truss work G G G G and E^1 E^1, diagonally braced, rigidly supporting the wings to about a quarter of their length out from the central frame (Fig. 4 and Plates 6, 7, 10 and 11). This truss work has been referred to by Dr. Zahm as being for the support of the widely separated front floats, but as this extended truss work was also employed under the rear wings, where only central floats were employed, it cannot be correct to say that this extended truss work was for the sole purpose of carrying widely separated floats. This can be clearly seen in practically all of the photographs shown of the Hammondsport machine.

7. The ribs of the Hammondsport machine were made heavier than the box construction of the Langley machine, and were stronger.

8. Hydroplane floats H H, unknown in the day of Langley and developed between the years 1909 and 1914, were substituted for Langley's original method of launching (Zahm Affidavit, p. 5).

9. The large fixed keel vertical surface (K, Fig. 1) situated below the frame in the Langley machine was entirely omitted in the Hammondsport machine. (Plates 7, 8, 9, 10, 12, 14 and 15.)

10. The small split vane steering rudder (V, Fig. 1) of the Langley machine was replaced with a single rudder (V, Fig. 2) of about one and a half times the area. The small vane rudder of the Langley machine was adjusted from a self-locking hand-wheel, and was intended to steer the machine slowly to right or to left (Langley Memoir, pp. 214, 216), while the large single surface rudder in the Hammondsport machine was connected part of the time to the regular Curtiss wheel, part of the time to the Curtiss yoke, and was used for maintaining lateral balance (Zahm Affidavit, p. 6; Zahm Report, p. 219), a system of control universally used to-day but entirely unknown to Langley.

11. The Penaud tail in the Hammondsport machine, instead of being merely adjustable up and down by a self-locking hand-wheel, as arranged by Langley (Langley Memoir, p. 216), was coupled to a regular Curtiss steering post in the Hammondsport machine (Zahm Affidavit, p. 5 and Plate 7), and was attached to an upstanding bracket, instead of to a depending bracket, as used by Langley, which gave it a position about 20ins. higher than in the Langley machine.

12. The original carburettor on the Langley engine was changed for one of modern type at Hammondsport. (Butman in Aviation, November 15th, 1917.)

13. The radiator employed by Langley was changed in the Hammondsport machine for a radiator of modern type. (Plates 6, 7 and 11.)

14. The forward corners of the original Langley propellers were cut off, after the manner of the early Wright propellers (see Plates 6, 7 and 8).

15. The Penaud tail rudder R on the Hammondsport machine was made turnable about a vertical axis (Zahm Affidavit, p. 7), a movement not intended by Langley in the machine of 1903. (Langley Memoir, p. 214.) This tail rudder was connected to a regular Curtiss steering wheel.

Although the machine was altered as above described, Dr. Zahm tells us in the affidavit which he afterwards made that he had frequently seen the original Langley machine in the Smithsonian Institution, and he was thoroughly familiar with its construction and operation; and he avers that

> "The machine . . . was the same machine in construction and operation as the original structure. The frame was the same; the engine was the same; the horizontal and vertical rudder located at the rear was the same; the vertical rudder under the machine was substantially the same; the propellers were the same and the wings were identical in construction

with the original machine, except, as I have heretofore stated, they were perhaps a little more roughly built and a little heavier." (Zahm Affidavit, p. 5.)

This statement does not accord with the facts as I have just shown them—most of which facts are clearly corroborated by the photographs here reproduced.

After making the aforementioned alterations, Dr. Zahm tells us in his official report to the Smithsonian Institution that the machine "rose in level poise, soared gracefully for 150 feet, and landed softly on the water." I was at Hammondsport in 1914 within two weeks after the last tests were made in which the Langley motor was used. I could not find any photos among the many photos on sale to show the machine in flight. I was told, however, that the machine did actually make short hops, due to its initial speed, and I have no reason to doubt that this was true. This, however, does not prove that the machine was capable of "sustained free flight," since it is well known that a machine incapable of sustained flight may make hops.

Several so-called flights are stated to have been made with this Hammondsport machine, which Dr. Zahm refers to as "the Langley aeroplane, kept as nearly as possible in its original condition," but they were obviously only hops over the surface of the water, and in none was "sustained free flight" attained, because, as Dr. Zahm states in the Affidavit which he made some time later, "it was apparent that owing to the great weight which had been given to the structure by the adding of the floats, it was necessary to increase the propeller thrust." (Zahm Affidavit, p. 8.)

Dr. Zahm here admits that the machine was not really in flight, because if it had been flying more propeller thrust would not be necessary in order to make it fly.

From the evidence that I have already brought to your attention, I submit that two facts are clearly proved:—

(*a*) The Langley machine was not capable of sustained free flight.

(*b*) The Langley machine was not successfully flown at Hammondsport, New York, on June 2nd, 1914.

The inscription on the Langley machine should therefore be changed to accord with the facts.

It having been shown in these early tests that it was necessary to increase the propeller thrust before a real flight could be made, the Langley engine and propellers were removed from the Hammondsport machine and a modern engine with a modern propeller were installed. This entailed a number of further alterations, a part of which I will mention.

16. The Langley engine, which in the Langley Memoir is said to have given 52.3 h.p. on the bench for ten hours, was now removed, and a Curtiss 80-100 h.p. motor was substituted (Zahm Affidavit, p. 8) and placed about 3½ft. farther forward on the machine (Fig. 2, and Plates 11 and 14).

17. The Langley modified twin propellers were removed and a modern tractor propeller, designed with knowledge acquired since Langley's day, was mounted in front of the machine (Zahm Affidavit, p. 8), about 16ft. forward of the location of the Langley propellers (Fig. 2, and Plates 11 and 12).

18. The pilot's car was now removed and the pilot's seat was placed about 14ft. farther back and about 3ft. higher than the seat in the Langley machine (Zahm Affidavit, p. 9, Fig. 2, and Plates 12, 13 and 14).

It was only after these further alterations and with the more powerful motor that hops of more than five seconds' duration were made. Dr. Zahm in the Smithsonian Report, p.221, in describing these later alleged flights on October 1st, 1914, says:—

"Hovering within 30ft. of the water, and without material loss of speed,

it made in quick succession flights of the following durations . . . 20 seconds, 20 seconds, 65 seconds, 20 seconds, 40 seconds, 45 seconds. . . .

"Apparently the machine could have flown much higher, and thus avoided touching the water during the lulls in the breeze; but higher flying did not seem advisable with the frail trussing of wings designed to carry 830lbs. instead of the 1,520lbs. actual weight."

Dr. Zahm suggests that there was loss of speed and that the machine touched the water during the lulls in the breeze. We may take it that the machine did not fly but that it made long hops during the gusts and came to the surface of the water with each lull in the wind.

Dr. Zahm was perfectly well aware at the time he made this statement, that the frail trussing of the wings designed by Langley, was not used on the Hammondsport machine. The trussing of the Hammondsport wings was made under Dr. Zahm's observation, and he knew when he made his report to the Smithsonian Institution that the trussing of the wings was not the Langley trussing designed by Langley to carry 830lbs. We know that Langley's wing trussing was frail. That was the reason Dr. Zahm and Mr. Curtiss did not adopt it in building up the Hammondsport machine.

It is interesting to know that on June 4th, 1915, more than a year after the so-called flight of June 2nd, 1914, attempts were still being made to fly the Hammondsport machine. The inverted V-frame over the rear wings was removed and the upper portion of the original Langley guy post was reinstated. Plate No. 15 is from a photo taken on June 4th, 1915, and shows this reinstatement. The following day, at about 10 o'clock in the morning, the machine was taken out and an attempt was made to fly it. After a run of several hundred feet, and before the machine had lifted from the water, the rear wings collapsed as they had done in Langley's day. So far as I am aware no further attempt was ever made to show that the Hammondsport machine was capable of flight. I am sorry not to have a photo of this wrecked machine to show you. Photographs were made of the wreck, but the films were taken from the photographer by the men in charge of the experiments. When the photographer protested that he had been given permission the day before to take photographs of the machine, he was told that while that was true, no photographs of the wrecked machine could be permitted on account of "legal complications."

SUMMARY.

It is untrue to say that Langley's machine of 1903 ever has flown or ever could fly. In both trials in 1903 the wings collapsed through faults in design of the machine, and not from any failure in the launching mechanism.

The machine used at Hammondsport in 1914 differed from the original Langley machine in many important respects.

The wings were of different area, different camber and different aspect ratio.

The system of wing trussing, which in the Langley machine had failed, was completely changed at Hammondsport.

The large keel surface of the Langley machine was altogether omitted.

The Langley system of launching was abandoned, and a system developed after his death was used in its place.

The original Langley propellers were modified and afterwards superseded by a modern propeller, based on knowledge not possessed by Langley.

A system of lateral control unknown to Langley was added. The dihedral angle of the wings on which Langley relied entirely for maintaining lateral balance was supplemented in the Hammondsport machine by the action of a rudder of increased size used as an aileron.

The steering wheel, post and shoulder yoke of a modern Curtiss machine were installed complete in the Hammondsport machine.

The original Langley engine of 52 h.p. was at first modified and afterwards superseded by a modern Curtiss motor of 80-100 h.p.

At first it was necessary to change the machine to carry the engine, and then it was necessary to change the engine to carry the machine. Finally there was neither the original Langley engine nor the original Langley machine.

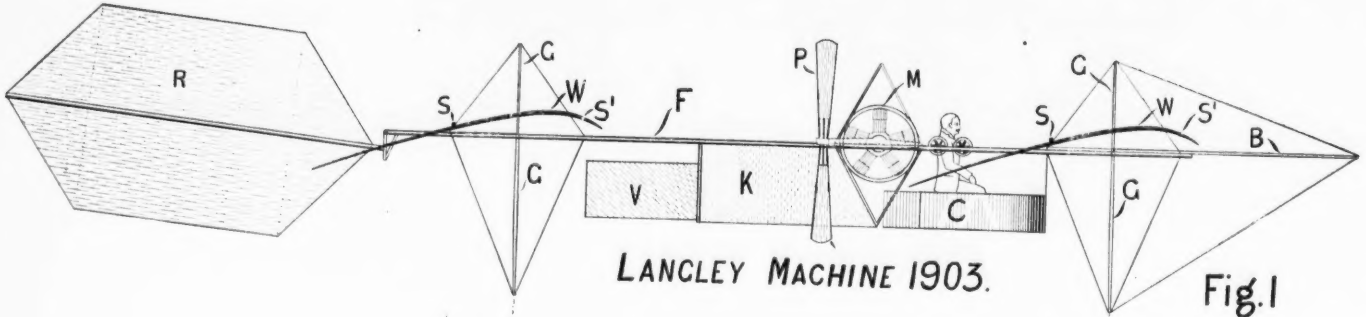
The remains of the Langley machine were at last returned to the Smithsonian Institution, and were entrusted to Mr. Reed for restoration to the original form of 1903. Mr. Reed did his work well, for when I saw the rebuilt Langley in the National Museum last April, it was very much like Langley built it. The wings had their leading edges restored, thus bringing them back to their former camber and area; the wings were stayed to the old guy posts, which were set forward again to their original but ineffective positions; the Langley propellers were restored; the pilot's car was again under the forward part of the frame; the Curtiss control gear had been removed. In fact, the whole machine had lost all appearance of having been out on an indiscreet adventure. (Refer to Plate No. 16.)

The present paper, however, does not deal with Langley's work except so far as is necessary in order to disprove the wild assertions made by pretended friends in order to build up for their own ends a credit for Langley which could not be enduring, a credit which Langley would be the first to repudiate were he here to do so. No fraud can long endure when so much evidence is on record. If it had not been my privilege to bring these facts before you, some other inquirer would have dug up the evidence at some later time and done the service to Langley I have now done in separating his upright life from association with anything which is not strictly true or honourable.

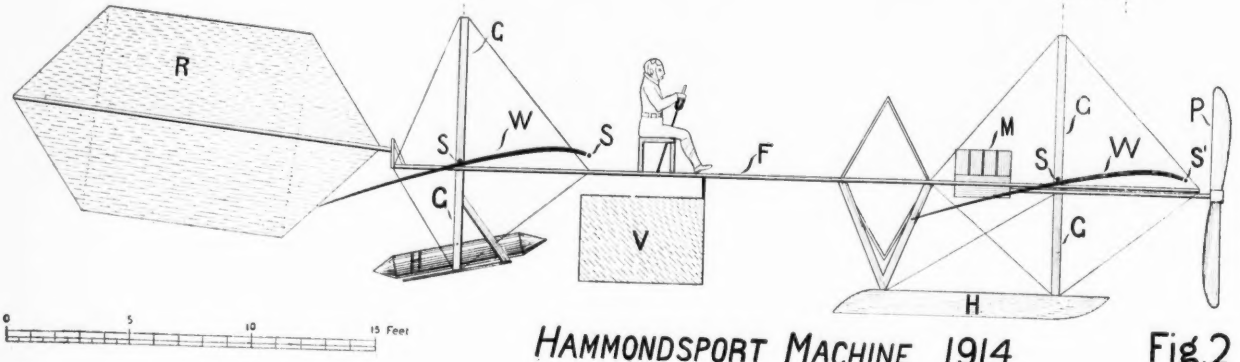
DISCUSSION.

Colonel A. Ogilvie said he had not much to say about this question, but he would like to pay a tribute to the lecturer himself and to say how much they who knew him intimately respected him for the efforts he had made in the vindication of the Wrights in Europe, and particularly in this country. It was not too much to say that if the Wrights had not had a man of his calibre to defend them from the many attacks that had been made over here, their name would not be so widely respected as it was. As to the actual substance of the lecture, he thought Mr. Brewer had proved his point very clearly, and to anyone who was at all accustomed to aeroplane construction or experiment it was perfectly clear that the original machine would have broken, as it actually did break, and that the altered Hammondsport machine was quite different.

Mr. F. Handley Page said he felt in a little quandary in discussing this paper, as he happened to know the parties on both sides, having this year had the pleasure of staying with Mr. Orville Wright and also of having dined with Dr. Zahm. In addition to that, his own Company had had legal proceedings under the Wright patent in America. When one came to express an opinion as to what were the rights or wrongs of the case, it was rather difficult to be what he might call neutral. There were so many points on Langley's original machine which differed from his experimental work that one rather wondered whether there was some experimental laboratory work unpublished, work possibly carried out after that already published and before the tests on the actual full-size machine. He did not know whether Mr. Griffith Brewer could inform them further on the point. All Langley's original experiments were carried out with



LANGLEY MACHINE 1903. Fig.1



HAMMONDSPORT MACHINE 1914. Fig.2

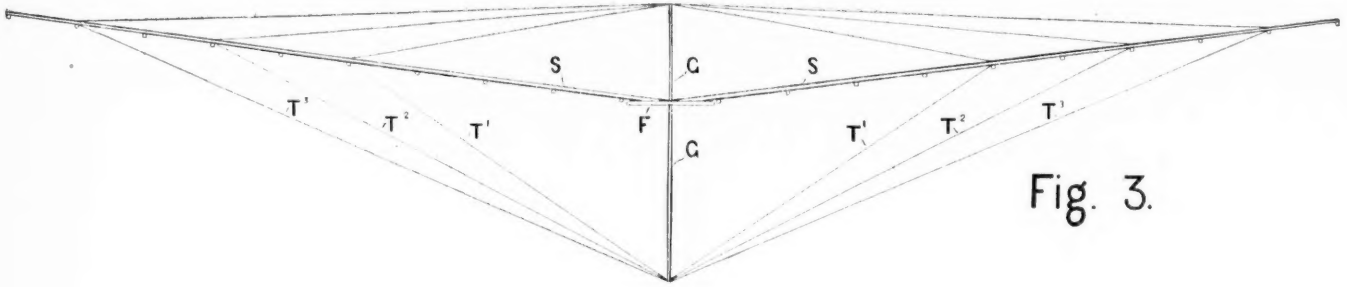


Fig. 3.

LANGLEY WING TRUSSING 1903.

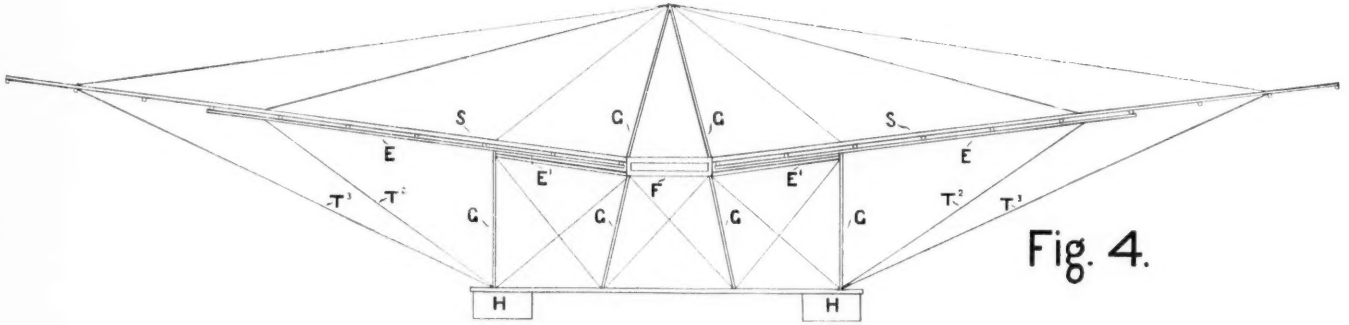
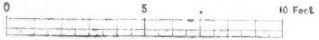
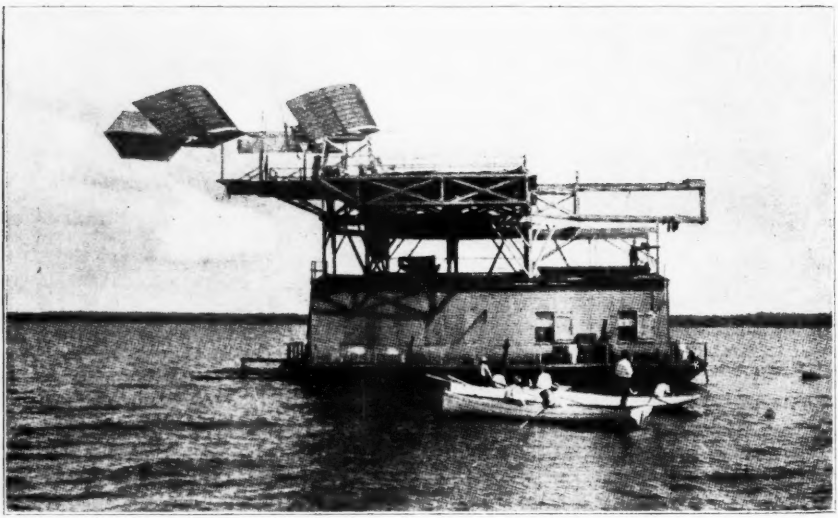


Fig. 4.

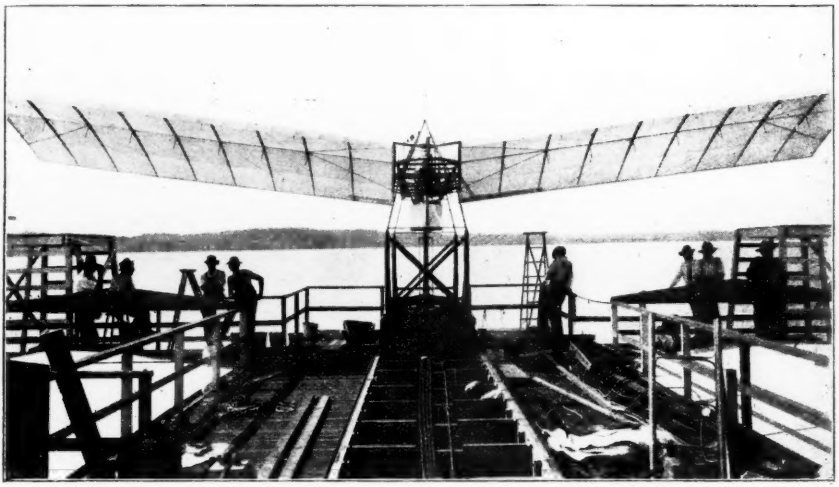


HAMMONDSPORT WING TRUSSING 1914.



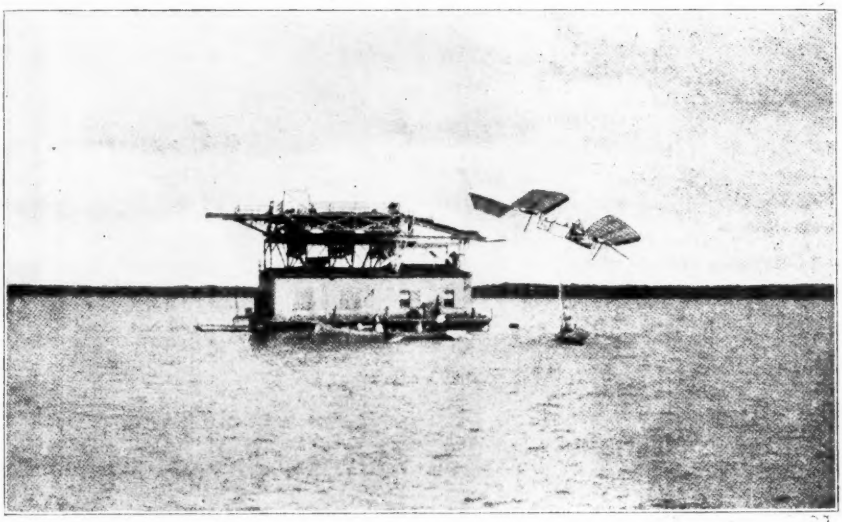
(Photo, Langley Memoir.)

PLATE I.—Langley Machine on houseboat, Potomac River.



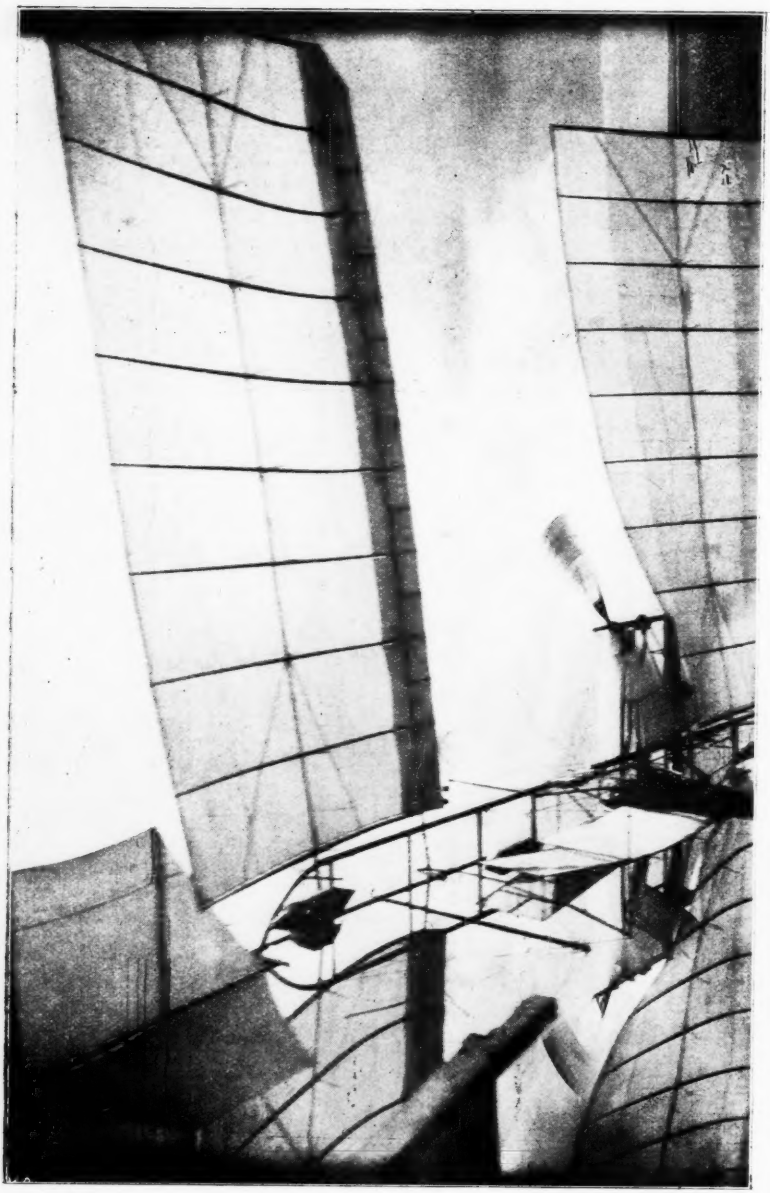
(Photo, purchased April, 1921, Smithsonian Institution.)

PLATE 2.—Langley Machine on launching carriage.



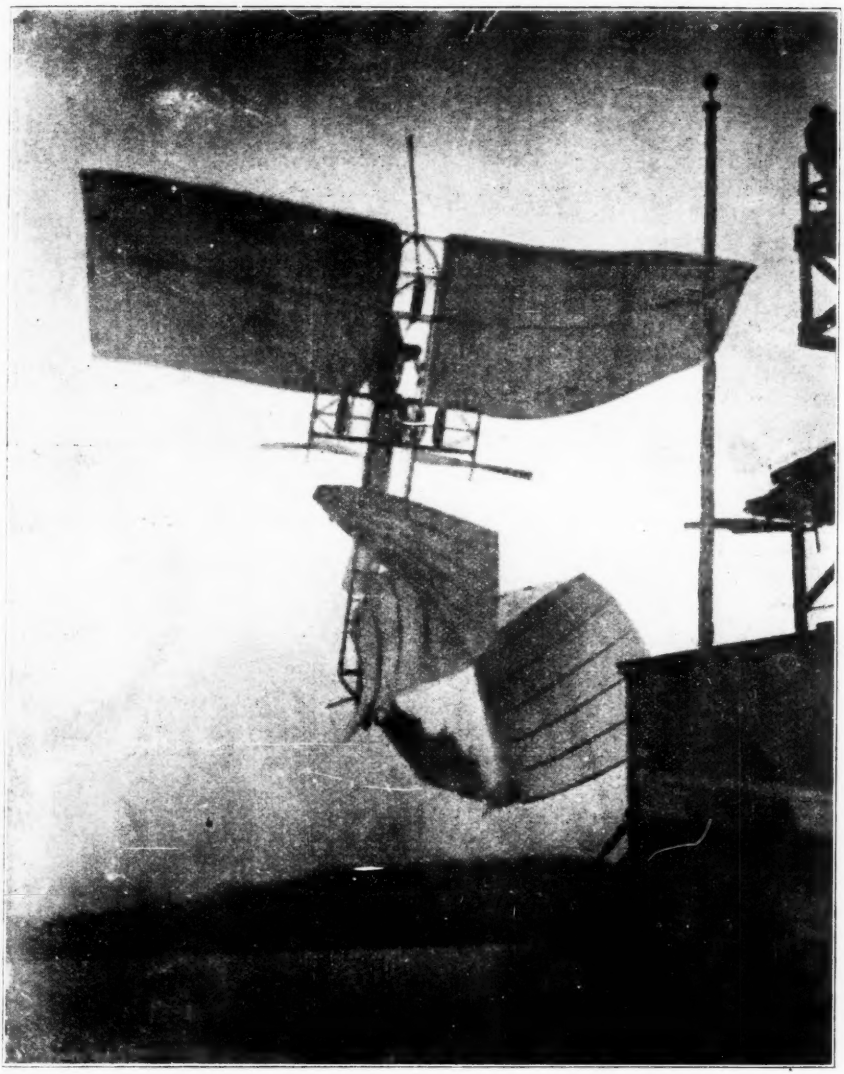
(Photo, Langley Memoir.)

PLATE 3.—Langley Machine falling from launching track into Potomac, 7th October, 1903.



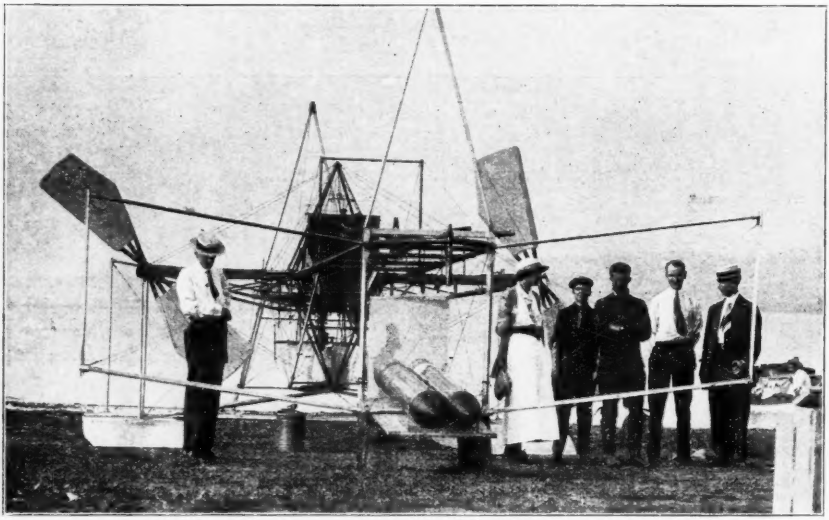
(Photo, purchased April, 1921, Smithsonian Institution.)

PLATE 4.—Langley Machine viewed from below, front wings twisted and bent against framework.



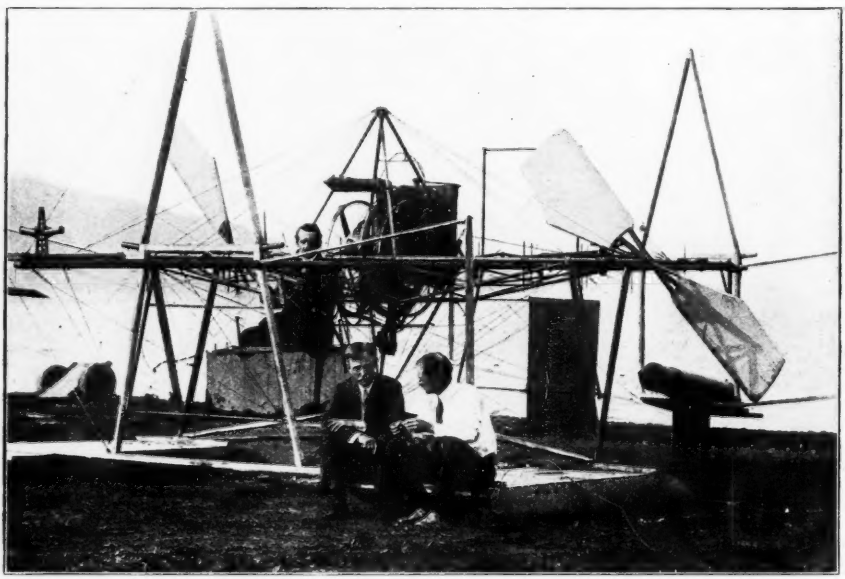
(Photo, purchased April, 1921, Smithsonian Institution.)

PLATE 5.—Langley Machine falling from launching apparatus, 8th December 1903, into Potomac. Note front wings distorted and spars broken, central frame bent.



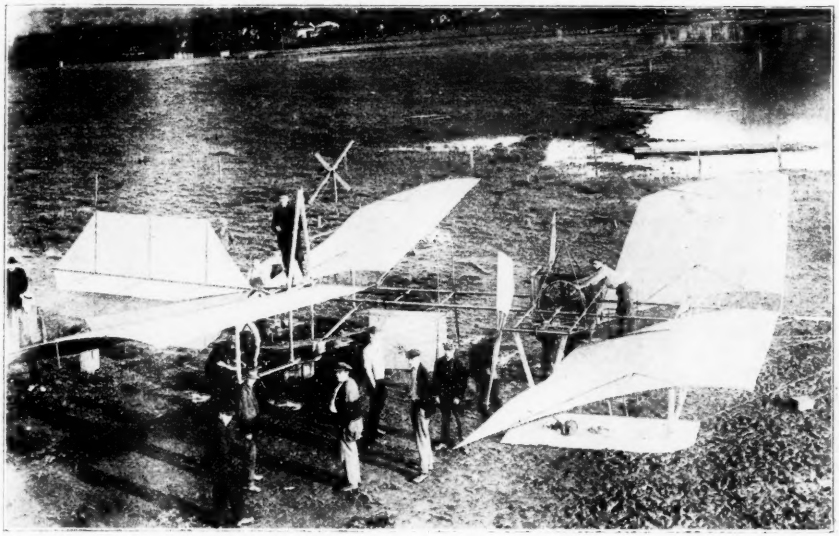
Dr. Walcott. Miss Walcott. Mr. Curtiss. Dr. Zahm.
(Photo, Benner.)

PLATE 6.—Hammondsport Machine with wings removed. Note additional spars and trussing for supporting wings.



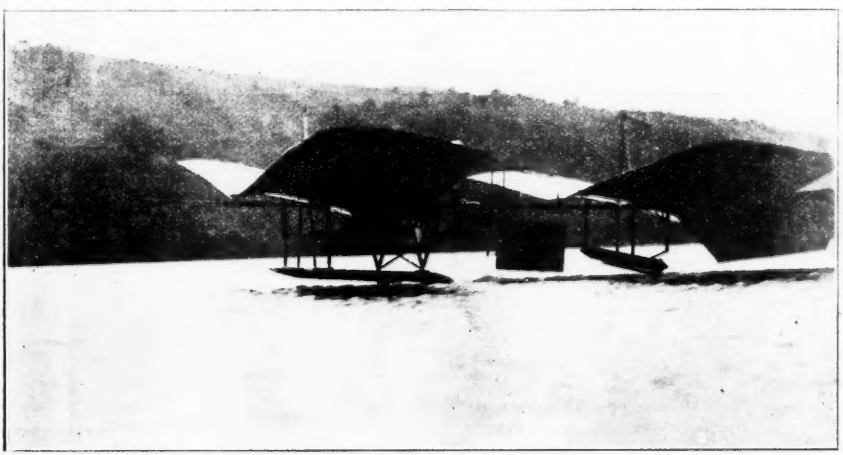
Manly. Zahm. Curtiss. *(Photo, Benner.)*

PLATE 7.—Hammondsport Machine. Note high A frames taking the place of Langley's short upper guy posts.



(Photo, Smithsonian Report.)

PLATE 8.—Hammondsport Machine. Note leading edge of wings terminating at front spars.

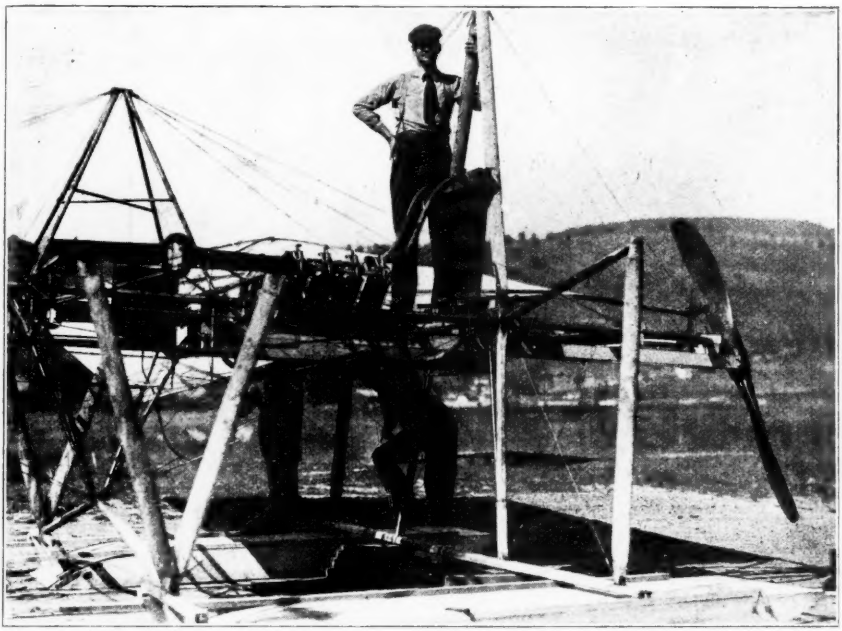


(Photo, Smithsonian Report.)

PLATE 9.—Hammondsport Machine on lake 2nd June, 1914.

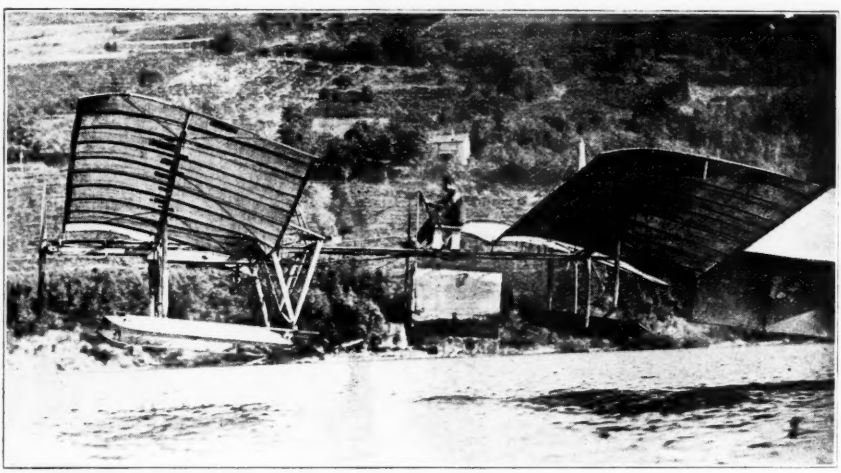
(Photo, Griffith Brewer.)

PLATE 10.—Hammondsport Machine, wings removed.



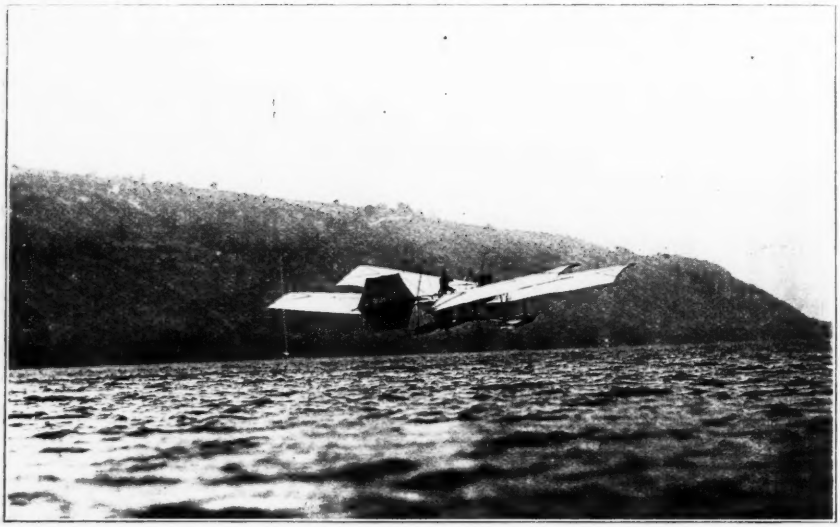
(Photo, Smithsonian Report.)

PLATE 11.—Hammondsport Machine with Curtiss motor, propeller, and modern radiator installed.



(Photo, Zahm Affidavit.)

PLATE 12.—Hammondsport Machine with Curtiss motor. Note position of pilot over frame, and front spars forming leading edges of wings.

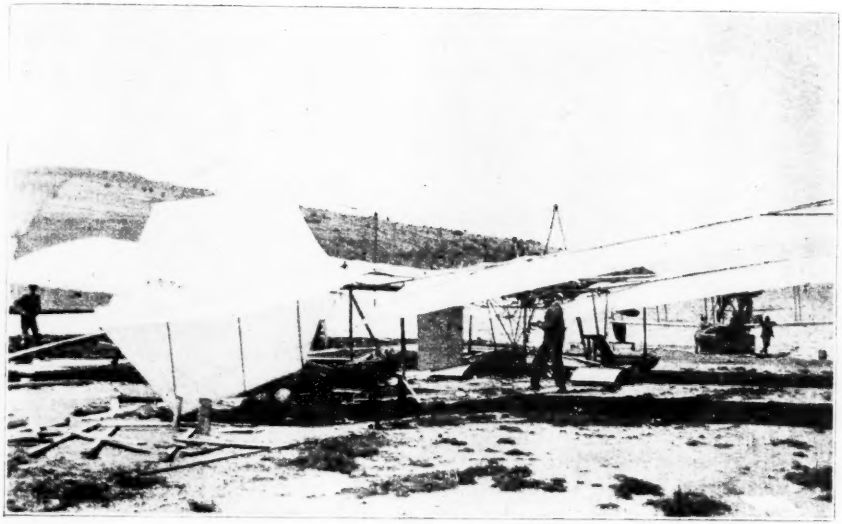


(Photo, Smithsonian Report.)

PLATE 13.—Hammondsport Machine over lake, 17th September, 1914.

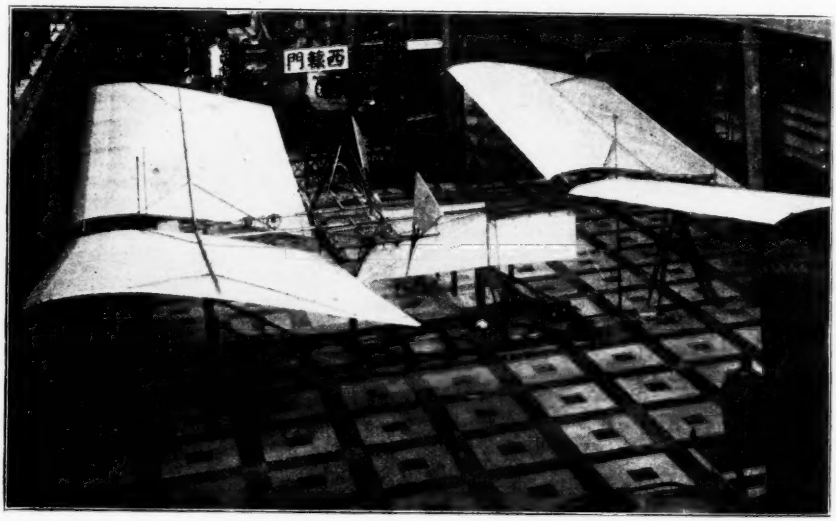
(Photo, Benner.)

PLATE 14.—Hammondsport Machine. Note further change in position of pilot.



(Photo, Lorin Wright.)

PLATE 15.—Hammondsport Machine, 4th June, 1915. Note reinstatement of short upper post over rear wings.



(Photo, purchased April, 1921, Smithsonian Institution.)

PLATE 16.—Reconstructed Langley Machine in Smithsonian Institution.

flat planes, as far as he was aware, and none of his laboratory work was with curved planes, whereas in the machine shown in the pictures by the lecturer, there curved planes were used of the order of 1 in 12, similar to that which Otto Lilienthal had in Germany. One wondered, therefore, whether there was not a gap in his experimental work which was not recorded.

In the present paper they had a good record of the historic research into what was and what was not done, and, as Colonel Ogilvie had said, the Wright brothers were fortunate in having so able and persistent an investigator as Mr. Griffith Brewer. He had unearthed a good deal of data which would otherwise have been completely lost. It seemed strange to think of procedure similar to what was adopted in America being carried out here. Could one imagine, for instance, two rival patentees discussing the steam engine and going to the South Kensington Museum, or somewhere else, and having out the original Watt engine, or some similar engine, and attempting to run it on the Great Western Railway to prove their point. To his unenlightened and English way of thinking it seemed extraordinary that a historical specimen of a machine should have been taken out from the Museum, where it very properly rested, and used for the purpose of determining whether or not the warping as laid down by the Wright patent was an essential feature or not, for the purpose of vindicating the Wrights' or Curtiss's claim. It seemed an extraordinary procedure to have adopted.

He thought everyone, from a historical point of view, owed a great debt of gratitude to Mr. Brewer for going so patiently and persistently through the details of all the experiments that were made.

The CHAIRMAN said, on behalf of the members, he moved a vote of thanks to the lecturer for his very interesting lecture. He had added to the sum of their aeronautical knowledge, and of human knowledge as well. As he had said, the lecture dealt with facts, and he (the Chairman) had no doubt the facts were as Mr. Brewer said. They would look forward with the greatest interest to any reply that might be received from the other side of the Atlantic.

From Dr. WALCOTT.

Smithsonian Institution,
Washington, U.S.A.,
October 10th, 1921.

Dear Sir,—With reference to the paper of Mr. Griffith Brewer on which you are so obliging as to request my comment, I regret very much that having been inaccessible in the field until October 9th I have not had time to take up the matter as thoroughly as I would have liked to do.

I take pleasure in saying at the start that the Smithsonian Institution fully recognises the well-deserved success of the Wright brothers in being the first to make actual flights in heavier-than-air power propelled machines. This recognition the Institution was among the earliest to make in a formal manner by an award of merit. This being so, I am not prepared to concede that the Wrights were the first to construct such a machine capable of such flight.

Mr. Brewer appears to claim :—

1. The Hammondsport experiments were made for Curtiss' ends at Curtiss' expense.
2. The changes made in guying the wings were indispensable to prevent their collapse.
3. The changes made in the engine were indispensable to make it run.
4. The changes made in the propellers were indispensable to give them sufficient thrust.

5. The changes made in the form of the wings were indispensable to flight.
6. The changes made in the rudder were indispensable to flight.
7. The Hammondsport tests of the original machine did not result in flight but merely in hops.

Mr. Brewer is misinformed in regard to some points, and I think imparts a false impression regarding others. I take up the points in the order just given.

I. I quote from my letter to Mr. Curtiss of March 31st, 1914:—

"In connection with the re-opening and development of work under the Langley Aerodynamical Laboratory, it seems desirable to make a thorough test of the principles involved in the construction of the Langley heavier-than-air man-carrying flying machine, especially the question as to the tandem arrangement of the planes, and general stability, especially longitudinal stability.

. . . . . .

"After my recent conference with you, I am writing to say that there will be an allotment of $2,000 made from the funds of the Langley Aerodynamical Laboratory for the purpose of conclusively testing the Langley machine and the principles involved in its construction. If you care to undertake such experimentation, will you answer at your earliest convenience? All materials connected with the original machine will be placed at your service for the purpose."

The Smithsonian Institution has still on file Mr. Curtiss' receipt for the payment made him in accordance with the above proposal.

II. Photographs show that in the original tests of the Langley machine, October 7th, 1903, one pair of wings stood up wholly without appearance of collapse after the machine took the air at full speed. Moreover, in the test of October 7th, 1903, the photographs show that the other pair was only slightly changed, and this change occurred for a very well attested reason, quite other than from their lack of efficient guying.

Mr. Brewer, in ignoring what follows on the same page, quite unfairly as it seems to me, quotes Mr. Manly's first statement to the newspaper reporters who beset him even before he was out of the water and who had to be pacified. Mr. Brewer should recognise that the succeeding well-considered statement by Manly and Langley after investigation of the causes of the failure of the test is the only one deserving of any weight:—

"After recovering the machine the foreman of the workmen (Mr. Reed) (who together with Mr. McDonald were the only ones on top of the boat when the launching actually took place), busied himself to discover what had caused the jerk to the machine at the moment it was released, which had been immediately followed by the great depression of the front end. After some little time he discovered that the upright guide at the extreme front of the launching car (which, as heretofore stated, was slotted to receive a metal lug projecting from the end of the guy-post, and thus prevent the front end of the framework from being twisted by a side wind striking the machine while it was still on the launching car) had been distorted, and the metal cap on it being stretched out of shape in a way which indicated that the pin of the front guy-post had hung in the cap, and that the guy-post was not therefore free from this part of the car when the end of the launching track dropped. The shock which the writer felt at the moment of launching and which had also been seen by others to occur was thus conclusively shown to have been due to the falling track, dragging the front end of the machine down with it. As the machine was travelling forward and the car had been almost instantly brought to a standstill by its buffer pistons co-acting with the buffer cylinders at the foot of the track, this front guy-post had been pulled backwards, and thus not only pulled the main guy-wires of the wings backwards and thereby

depressed the front edge of the front wings so that they had no angle of inclination, but had also bent the front end of the metal framework downward—effects which were discovered from the later examinations of the frame and the guy-post itself. From the instantaneous photographs which were obtained, indisputable evidence was obtained that this was what actually occurred. . . .

" After completing the recovery of the machine and the examination as to the extent of the injuries it had sustained, and finding unquestionable evidence that the accident had been caused by the front guy-post hanging in its guide block on the launching car, the workmen were set to work straightening out and arranging the various parts, fittings and accessories, and cleaning up the engine which fortunately had sustained no injury whatever. After a consultation in Washington with Mr. Langley, who had been unable to be present at the experiment, both concerning what had already occurred and also what should be done regarding the future of the work, and in view of the fact that the statement which the writer had given to the Press representatives, immediately after the accident, had been made before there had been time to make an examination of the machine itself, it was decided that it would be best to give to the Press a short statement to correct the earlier one, and Mr. Langley accordingly made public the following note :—

" ' Mr. Langley states that he was not an eye-witness of the experiment at Widewater yesterday, having been detained in Washington by business, but that on the report of Mr. Manly, immediately in charge, he is able to say that the latter's first impression that there had been defective balancing was corrected by a minuter examination, when the clutch, which held the aerodrome on the launching ways and which should have released it at the instant of the fall, was found to be injured.

" ' The machinery was working perfectly and giving every reason to anticipate a successful flight, when this accident (due wholly to the launching mechanism) drew the aerodrome abruptly downward at the moment of release and cast it into the water near the houseboat. The statement that the machine failed for lack of power to fly was wholly a mistaken one.' "

In view of these facts, I believe the photographic evidence of Langley's own tests of the great machine show that Mr. Brewer is wrong in maintaining that the Langley wings lacked efficient guying to prevent their collapse.

But one who knew Langley's work and the enormous mass of still unpublished tests of the wings, on the whirling table, with sand loading, and otherwise, and the experiments on lift, centre of pressure, and resistance for curved surfaces which occupied Mr. Huffaker many months, knows that Langley and Manly had sound experiments behind all they did and had not neglected to learn either the proper place or the sufficient strength for the guying.

III. The facts about the engine are quite opposite to Mr. Brewer's suggestion. In 1903 the engine ran far better than in 1914. There are many witnesses here who know that it ran with all five cylinders speaking for hours and hours at a time in 1903, delivering the full brake horsepower claimed by Manly. The Curtiss mechanics were unfamiliar with it, Mr. Manly was not present to instruct them until they had materially injured it by their unfamiliarity, and it never worked at Hammondsport anywhere nearly as well as in 1903.

IV. The propellers were cut down at Hammondsport, not because they could not hold up, but because the engine worked so poorly there that it did not run to speed with them. The thrust of the propellers at Hammondsport never exceeded 325 pounds, while in 1903 it was measured over and over again at 450 to 475 pounds with the original propellers.

V. The wings made at Hammondsport were far inferior in workmanship to Mr. Reed's wings. That they lacked the additional supporting surface of the overhang was to save time and expense, not for better design.

VI. Langley's whole energy was thrown to the single aim of making a single flight and a safe landing. He knew well that if this was accomplished the means to continue would be available. Owing to the wreck of October 7th, 1903, his means were exhausted before he got a flight. The rudder used in the 1903 tests was, as previous flights with the quarter size models had shown, adequate to guide a flight even without a pilot.

VII. I was present at Hammondsport on May 31st, 1914, and saw the machine with the original engine* giving only two-thirds the original thrust and with wings approximately of the original design, but far rougher executed, get under way from rest and fly gracefully, carrying, besides a man, over 300 pounds of floats in excess of what the machine was designed to carry. I am still confident that what it did under these relatively adverse circumstances is far inferior to what it was capable of doing in its original condition.

Very truly yours,
CHARLES D. WALCOTT,
Secretary.

Colonel W. Lockwood Marsh,
Secretary, Royal Aëronautical Society,
London, England.

* Mr. Brewer's Figs. 1 and 2 are wholly misleading. The earlier flights of 1914 were made with the original engine, the original propellers, and original cockpit. The later introduction of the Curtiss engine, traction propellers, etc., was for the purpose of gaining the further aerodynamical knowledge mentioned in my above cited letter of March 31, 1914.

From Dr. A. F. ZAHM.

REVIEW OF EXPERIMENTS WITH THE REHABILITATED LANGLEY AEROPLANE IN 1914.

Origin and Motive of the Experiments.

The experiments with the rehabilitated Langley aeroplane, made by Mr. G. H. Curtiss, at Hammondsport, N.Y., in 1914, were initiated by the Smithsonian Institution for purely scientific purposes, were conducted at its expense, and were reported by its delegated observer, the Recorder of the Langley Aeronautical Laboratory, who then had no connection, either actual or prospective, with the Curtiss Aeroplane Co. These tests were no more initiated for the purpose of patent litigation than were Langley's original experiments, though both were later cited for that purpose.

The statement that none of the costs, apart from the carriage of the machine to Hammondsport, have ever been paid by the Smithsonian, is the irresponsible gossip of a partisan who could easily have ascertained the truth. To impugn the motives of the Smithsonian men associated with the work of retesting the Langley aeroplane in 1914 is a discourtesy and injustice that well might be discountenanced by an impartial society. Aside from the innuendoes and direct imputations, is it quite decorous to regale an honourable body of men with isolated bits of testimony selected from a compromised patent unit by a special pleader who paid a friendly visit to the complainant, but failed to interview the accused? Surely the members of a royal society cannot relish the false report of a partisan member upon the financial conduct of an honoured institution of a friendly nation.

Main Objects of the Tests.

As stated in the Smithsonian Annual Report for 1914, "The main objects of these renewed trials were, first to show whether the original Langley machine was

capable of sustained free flight with a pilot, and secondly, to determine more fully the advantages of the tandem type of aeroplane. . . . To achieve the two main objects above mentioned, the aeroplane would first be flown as nearly as possible in its original condition, then with such modifications as might seem desirable for technical or other reasons."

As the structural strength of the original machine had not been seriously questioned, no effort was now made to prove its adequacy. The prime effort was to prove (1) that the original machine could be propelled with its own power, (2) that it could be controlled in flight with the devices provided by Secretary Langley.

Claims of Other Aeroplane Inventors.

In the trials and reports of 1914 no attempts were made to belittle the work of other inventors. It was not claimed that Langley invented *the* aeroplane, or that he had brought in the era of practical flying. On the contrary, the printed report states that " the aeroplane as it stands to-day (1914) is the creation not of any one man, but rather of three generations of men. It was the invention of the nineteenth century; it will be the fruition, if not the perfection, of the twentieth century. During the long decades succeeding the time of Sir George Cayley, builder of aerial gliders and sagacious exponent of the laws of flight, continuous progress has been made in every department of theoretical and practical aviation—progress in accumulating the data of aeromechanics, in discovering the principles of this science, in improving the instruments of aerotechnic research, in devising the organs and perfecting the structural details of the present day dynamic flying machine."

The writer of that report had, in his book on aerial navigation, credited the English people with the invention of the chief essential elements of the aeroplane. Before the middle of the nineteenth century the English, notably Cayley and Henson, had provided these five essentials of a practical land aeroplane:—(1) A main sustaining plane suitably trussed and surfaced; (2) a vertical rudder; (3) a horizontal rudder or elevator; (4) an engine and propeller system; (5) a wheeled undercarriage.

With these five essential elements, as proved by many pilots, and notably by the French prior to the autumn of 1909, an aeroplane can make flights of more than an hour in fair or moderately rough weather.

The sixth essential element is the lateral rudder. This was invented and described by many persons prior to 1900. It received its first adequate experimental proof in the famous Kitty Hawk glider experiments, made at the beginning of the twentieth century.

Furthermore, the combination of said three rudders, in other words the three-torque system of control for an aeroplane, was invented and described by several persons prior to the year 1900. Goupil not only described the system in his book, but also embodied it in a proposed aeroplane which is now known to be operable under sufficient thrust.

What was Claimed for Langley.

The same report summarises the aerotechnic work of Dr. Langley as follows:—

" 1. His aerodynamic experiments, some published and some as yet unpublished, were complete enough to form a basis for practical pioneer aviation.

" 2. He built and launched, in 1896, the first steam model aeroplane capable of prolonged free flight, and possessing good inherent stability.

" 3. He built the first internal combustion motor suitable for a practical man-carrying aeroplane.

"4. He developed and successfully launched the first gasoline model aeroplane capable of sustained free flight.

"5. He developed and built the first man-carrying aeroplane capable of sustained free flight."

Items 1, 2, 3, 4, were merely stated, because they were incidental to the report and did not seem to require proof.

What the Tests of 1914 Proved.

Claim 5 was proved by the Hammondsport trials of 1914, except for the item of structural strength. Mr. Manly's estimates and sand load tests indicated that the original machine was strong enough. If there be still sufficient doubt among competent engineers, the wing strength can easily be subjected to static analysis and a load test. But the bare statement, without figures, that the original wings were too weak should not be accepted by fair-minded men.

How then did the 1914 tests prove that the Langley machine of 1903 had in the air sufficient thrust and control for sustained flight? The principle of the proof is this, from the short flights of an overloaded and less efficient plane one may infer the feasibility of a longer flight with a lighter and more efficient plane of the same type.

The rehabilitated Langley monoplane which made short flights on May 28th and June 2nd, 1914, was under the following handicaps as compared with the original: (1) It weighed 40 per cent. more; (2) its wings had about 3 per cent. less area; (3) its resistance was much greater; (4) its propellers in all the tests gave less than 90 per cent. of the thrust developed in the 1903 tests.

Mr. Manly, who personally overhauled the engine, complained that it was below its normal power, and laid this to lack of adjustment and to the inadequate carburettor. Mr. Curtiss had the propellers reduced at the leading corners to allow faster rotation, but secured thereby no material increase of thrust. True, the over-all aspect ratio had been increased slightly; but what was the resulting gain in efficiency—3 or 4 per cent.? Certainly this was a trivial increase compared with the large handicaps.

From the fact that this abnormally small propeller thrust sustained a short flight of a greatly overloaded machine, with greatly increased frame resistance, it was inferred that the greater thrust on the less resistant plane of 1903 was sufficient to propel it in a longer free flight.

If an eagle with some wing feathers gone makes a short flight with a child in his talons, a scientist infers the possibility of a longer flight with unimpaired feathers and without the child. This inference might not be indorsed by a patent attorney or a partisan having some business interest to subserve.

To control the longitudinal poise of the Langley aeroplane, once in the air, only movement of the elevator was necessary. The large steering rudder was useful in maintaining a definite course and in parrying wind gusts that tended to rock the machine. All this had been demonstrated with French machines not having ailerons.

On the water it was obviously impracticable to cross the wind with the big tail fixed because it acted as a weather vane. It therefore was made to swivel about the vertical axis provided for that purpose in the 1903 machine. At the same time the vertical rudder under the main frame was locked and held as a fixed fin or keel in all the 1914 tests after the first one. So adjusted the control surfaces were not materially different in arrangement and function from those of the original Langley scheme, if one excepts the possible use of the underneath rudder as an aileron of short leverage. The Langley keel surface aft of the pilot's boat could obviously be used or not with immaterial difference.

The whole balancing and guiding of the 1914 machine, after May 28th, was effected with the double tail provided by Mr. Langley, turning about the horizontal and vertical axes provided by him. The mechanism for turning it, whether stick or wheel, is known to be immaterial. The essential thing is that these two movements were a part of the design for both the 1903 and the 1914 craft, and that they controlled the latter successfully. One may therefore conclude that the original machine could be controlled in flight with the double rudder provided by Mr. Langley.

It is obvious also to present-day engineers that the little rudder of the original machine, though not tested in 1914, was competent to steer the craft in the air, the big rudder being used only to control the longitudinal poise. The split-vane rudder of 1903 was more powerful than its substitute in 1914, for each of its surfaces measured 11.5 square feet as against 12 square feet in the single surface substitute.

The flights made after installation of the Curtiss engine and tractor screw allowed a fuller testing of the control surfaces. In all these flights when the line of gravity of the machine passed through the central pyramid, as in the original machine, the statical balance and the aerodynamic control were satisfactory. The separation of the pilot and propulsive plant in the later tests did not affect the statical balance, and therefore need not be urged as a serious objection in a test of the means for maintaining the equilibrium.

In all the tests the same wings were used, and were made partly of the original Langley hollow wood ribs, partly of new and solid wood ribs of the same size and shape. The hollow ones were strong enough, but too few; the new ones were made solid for economy. These wings were of the exact size and shape of those made by Mr. Langley without the sharply arched leading edge, and were not materially better than his hooked-nosed ones.

The upper king-post, which in 1903 supported either pair of wings, could sustain an end thrust of more than a thousand pounds. The down pull on it due to the wing weight alone was less than a hundred pounds. Still persons ignorant of theoretical mechanics assert that it was too feeble. Perhaps that is why the practical mechanics substituted such monstrous wooden struts; perhaps, too, they thought of the pounding on the waves. The Langley king-post was a tough steel tube 109 cm. long; 2.2 cm. in outside diameter; 0.100 cm. in wall thickness. Euler's formula shows that it should support 1,400 pounds, and therefore have a large safety factor during a sustained free flight of the 1903 machine. Partisan wiseacres contend that it was inadequate. They use but empty words, not figures. One deponent, who stalls at elementary statics, solemnly swore the little strut was weak. And blind Maeonides with golden chain now leads blind Thamyris, who sings aloud that sacred history to soothe the soul of ages still to come.

Definition of Sustained Flight.

In a quiet atmosphere an airplane executes sustained flight so long as its energy remains undiminished. For example, while its speed and altitude remain constant, or increase, its flight is sustained; that is to say, is perpetuated independently of its initial momentum. Thus, when the Langley airplane, on May 28th, was flying at steady speed and height, driven by the original engine, its flight was sustained till Mr. Curtiss cut off the power. Later, when the Langley airplane, driven by a Curtiss engine, flew half a mile at practically undiminished speed and altitude, its flight was sustained. The first flights have been called "short hops," the later "long hops." American pilots speak of crossing the continent in two or three hops. "What are words but birds that fly through the air and hurt not a stone?"

Summary of Results.

(1) The short flights of the overloaded Langley aeroplane in May and June, 1914, proved the propulsive system of the 1903 machine powerful enough for sustained flight.

(2) The half-mile flights made later in 1914, as well as the earlier ones, proved that the control surfaces of the 1903 aeroplane were sufficient to balance and steer the machine in the air.

(3) All the flights indicated that the hollow wing ribs were strong enough for the 1903 machine, but proved nothing for or against the adequacy of the general wing trussing. A special wing test could easily decide this question, if further evidence beyond that supplied by the original Smithsonian records be still required by a representative body of competent engineers.

Laundry List Objections.

It seems superfluous to notice seriatim all the trivial differences a hardware clerk can find between the Langley aeroplane of 1903 and that of 1914. Multitudinous, minute, inconsequential, they read like a laundry list. And if a thousand objections were answered, a thousand more would follow. In the list of changes why should the colour escape attention? A canary yellow wing differs from a faded buff.

Conclusion.

The foregoing paragraphs, written on too short notice, treat some of the major questions at issue. If the summarised inferences cannot be admitted, neither have they been disproved. A strut is not weakened by denying its strength. The objections so far offered have little cogency, for they are mainly declaratory and statistical. They are the objections of a book-keeper rather than of an engineer. But they doubtless have as much potency as the arguments of those mighty mathematicians who used to prove the impossibility of sustained free flight in any possible aeroplane of man's contrivance. And they have the merit of separating Langley's upright life from association with anything which is not strictly true or honourable.

Epilogue.

The foregoing paper, prepared by invitation of the Council of the Royal Aëronautical Society, was forwarded to the Secretary with the following recommendation :—

" Permit me very respectfully to recommend to the Council that it rigorously delete from the papers presented in this case, pro and con, all harsh personalities, all innuendoes and imputations of bad faith, as unbecoming to the dignity of scientific discussion, and distasteful to the urbane and cultured audience of a royal society."

Cosmos Club, Washington, D.C.,
October 10th, 1921.

From Mr. C. M. MANLY.

I would have been amazed at the contents of this paper by Mr. Griffith Brewer had I not, upon reading it, immediately recalled the great similarity between a large portion of the statements made in it and the very bald statements made in the affidavit of Mr. Orville Wright in the suit of the Wright Company *v.* the Curtiss Aeroplane Company in 1915, in which Mr. Wright claimed for himself

and his brother the credit for everything that had been achieved in aviation from the beginning of time, and had dismissed everything that went before as " mere speculation and theory excepting," as he described it, " the desultory experiments of men like Lilienthal and Chanute with powerless gliding machines, and like Maxim, Ader and Prof. Langley, with miniature models and experimental full-sized machines which never flew, nor were capable of flight." In fact, it appears, upon a review of this affidavit of Mr. Orville Wright, that the present paper by Mr. Brewer is merely a condensed statement of the assertions, arguments, speculations and errors of this affidavit by Mr. Wright.

Before discussing Mr. Brewer's paper in detail, I wish to say that there is no one more willing and anxious to give due credit to Mr. Orville Wright and his late lamented brother, Mr. Wilbur Wright, for the brilliancy of their achievement in building the first machine that actually transported a human being through the air in controlled flight, as well as for being the first to actually fly in such a machine, and also for their great ability and pluck in achieving these results entirely through their own resources. I do deny, however, that the machine which they built and with which they accomplished the first flight was the first full-size machine capable of controlled flight, and I do assert that the Langley machine, which was built prior to theirs, was capable of successfully transporting a human being through the air and being controlled in such flight, and that it was prevented in 1903 from actually demonstrating its ability to achieve this result entirely through unfortunate accidents in connection with the two launchings which were made of it, and not through any inherent weakness, inefficiency, lack of power, or lack of any essential element necessary to the securing of such a result.

It must be remembered, in connection with the large Langley machine, that the general plans for it had been settled by Dr. Langley prior to my becoming associated with him in June, 1898. Also that these plans called for this large machine to be as near a duplicate as possible, on an enlarged scale, of the two steam-driven models which had so successfully flown in 1896 and which were approximately one-quarter the linear dimensions of this large machine. While Dr. Langley had, from the beginning of the undertaking, felt that it was necessary to replace the steam power plants of the successful models of 1896 with an internal combustion engine, and recognised the securing of such a power plant as being probably the most formidable obstacle to be overcome, still he felt that in an art which was so new and in which there was practically no other proved experience than these successful models to guide him, it was fundamentally important to rigidly adhere to the general design of the successful models, and not to change the design in any particular, except where absolutely necessary, at the same time that the scale of the machine was being changed. While, therefore, many proposals were made and discussed with reference to many of the features of the machine, which it was recognised would need to be changed before it would be thoroughly practicable for war or commercial purposes, still he resolutely brushed aside all such proposals that were not felt by him to be necessary in securing the one first object, that of enabling the machine to prove the practicability of transporting a human being through the air in controlled flight, by actually doing so under the guidance of such an operator. It is, therefore, to be understood that the Langley Memoir, to which Mr. Brewer refers, was prepared by me in such time as I could spare from an otherwise busy life, after ceasing my connection with the Smithsonian Institution, and that it was not intended as a treatise on the subject of aviation, but merely as an historical record containing such manuscript as Dr. Langley had himself, prior to his death, prepared, relating to the steam-propelled models, supplemented by a description of what seemed to me to be the more important constructions and tests carried out during the several years that I was associated with him. In fact, the draft of the entire manuscript which I prepared for this publication was completed prior to the fall of 1907, but

owing to the extreme pressure of other matters which could not be postponed, I was unable to complete the review and proof reading and release the manuscript for publication until I again took it up, in 1911, after a lapse of four years. The period of time, therefore, between the actual carrying on of the work and the writing of the manuscript record of it was not so great as would be supposed from the fact that the volume was not published until 1912.

It had been my hope and ambition, on closing up the work of Dr. Langley in 1904, as stated in the Langley Memoir referred to, to personally provide the necessary funds and to personally make a further test of the machine to demonstrate that it would fly successfully, exactly as it was attempted to fly it in 1903, and thus achieve what Mr. Brewer very aptly describes as "one of the most dramatic events in the history of aviation." However, fortune did not smile on me to the extent to enable me to personally meet the expense which such a demonstration would involve, and, consequently, I was very much pleased in 1914 when I learned that Dr. Walcott had arranged for the Langley machine to be tested by Mr. Curtiss at Hammondsport. Unfortunately I was not present at any of the tests of the Langley machine at Hammondsport, and therefore did not have the satisfaction of seeing it fly equipped with its original motor and carrying a load of approximately forty per cent. more than we had burdened it with in 1903. However, I did see the machine itself when I visited Hammondsport on June 30th, 1914, and was most bitterly disappointed to find not only that in rebuilding the wings such portions of the structure as had had to be supplied to supplement the spare parts which we had remaining after the second trial in December, 1903, were so crude and the wings so roughly assembled that it seemed hardly possible that the machine could be expected to fly with such crude and poorly assembled wings, but that the engine and transmission and propeller shafts had been so greatly abused by attempting to run the engine without having it in proper adjustment, that I was more than ever surprised that the machine would fly under such conditions carrying even its original weight, much less its greatly increased weight, as well as greatly increased head resistance, which had been added to it in mounting it on pontoons.

From what I did learn when I was at Hammondsport on June 30th, 1914, from personal interviews with various eye witnesses of the tests which had previously been made of the machine, I was convinced, however, that the machine had been successfully flown for short intervals on several occasions previously, and that this had been accomplished in spite of the fact that at no time had the original Manly motor, with which it was equipped, been operating at anything like the power that it was capable of giving, as shown by the previous tests in 1902 and 1903, this lack in power being due solely to the improper adjustment of the engine and its being equipped with a carburettor which was either not suited to it or not adjusted to give the best performance. When I left Hammondsport on this visit of one day only, I was assured that the defects in the adjustment of the engine would be remedied and the ball bearings on the transmission and propeller shafts, which had been damaged, would be repaired before further tests were made with it. I later understood, however, that the engine was removed and a Curtiss engine substituted in order to avoid both the danger of damage to the original engine and the delays which it was feared would be encountered in keeping the ball bearings of the transmission shafts in proper repair, and that as the original engine had already demonstrated its ability to successfully propel the machine, such further tests as were desired in connection with the study of the machine aerodynamically could be accomplished more expeditiously with an engine driving a single propeller without the use of gearing, etc., required by the original engine and its dual propeller arrangement.

Referring, now, more specifically to the criticisms which Mr. Brewer has made under the heading, "Original Langley Flying Machine, 1903," Mr. Brewer's statement in the third paragraph under this heading that the evidence

which he was presenting " is only a part of the evidence in my possession " is very apt indeed, for it is quite apparent that in attempting to prove his point he has deliberately suppressed part of the evidence which he had in his possession. For example, he has quoted in full from the Langley Memoir the statement which I made to the Associated Press, immediately after my emergence from the water and return to the houseboat, following the launching of the machine on October 7th, 1903, but has not quoted what is stated in the Memoir—that " although his (my) first inclination was not to make any statement (regarding the test) until a complete examination could be made to determine both the cause of the lack of success and also the extent of the damage which had been sustained by the machine, yet owing to the very great pressure brought to bear by the Press representatives, who said that unless some statement was given out they would write their own conclusions as to the cause of the mishap, he (I) finally gave out the following statement," the statement which he quotes. Nor has Mr. Brewer seen fit to quote you the immediately succeeding paragraph which reads as follows :—

" After recovering the machine, the foreman of the workmen (Mr. Reed) (who together with Mr. McDonald were the only ones on top of the boat when the launching actually took place), busied himself to discover what had caused the jerk to the machine at the moment it was released, which had been immediately followed by the great depression of the front end. After some little time he discovered that the upright guide at the extreme front of the launching car (which, as heretofore stated, was slotted to receive a metal lug projecting from the end of the guy-post, and thus prevent the front end of the framework from being twisted by a side wind striking the machine while it was still on the launching car) had been distorted, the metal cap on it being stretched out of shape in a way which indicated that the pin of the front guy-post had hung in the cap, and that the guy-post was not therefore free from this part of the car when the end of the launching track dropped. The shock which the writer felt at the moment of launching and which had also been seen by others to occur was thus conclusively shown to have been due to the falling track, dragging the front end of the machine down with it. As the machine was travelling forward and the car had been almost instantly brought to a standstill by its buffer pistons co-acting with the buffer cylinders at the foot of the track, this front guy-post had been pulled backwards, and thus not only pulled the main guy-wires of the wings backwards and thereby depressed the front edge of the front wings so that they had no angle of inclination, but had also bent the front end of the metal framework downward—effects which were discovered from the later examination of the frame and the guy-post itself. From the instantaneous photographs which were obtained, indisputable evidence was obtained that this was what actually occurred. Referring to the photograph, Plate 95, which was taken by Mr. G. H. Powell, Secretary of the Board of Ordnance and Fortification, and which shows the machine just a few feet in front of the point where it was actually launched, it will at once be seen that the front end of the frame is bent downward and that the front guy-post instead of being parallel with the rear one has been deflected backward at the lower end through an angle of thirty degrees. Referring further to the photograph, Plate 96, which was taken at the same instant as the one just described, it will be seen that even this one, which is a view of the machine as it passed almost directly over Mr. Smillie's head, most clearly shows the extreme extent to which the front wings had been distorted, the rear edges of the wings near the frame having been twisted up until they struck the cross-frame, and the outer ends being free to twist had been forced up very much higher."

Neither does Mr. Brewer, in his immediately preceding quotation from Mr. Reed's affidavit, see fit to give you sufficient from the said affidavit to enable you to see that Mr. Reed's use of the expression, " it appeared at first," refers merely to an instantaneous impression which he immediately corrected by making an

immediate examination of the launching car to determine what had caused the machine to plunge downward at so sharp an angle, and that it was he who immediately discovered that the front guy-post had hung on the launching car, as evidenced by the distortion of the metal fitting of the car on which the guy-post had hung. Furthermore, Mr. Brewer is very careful to call your attention to the distortion of the front wings, as shown in these two photographs and as described in the Memoir in the paragraph which he omitted, but he does not call attention to the fact, which was called attention to in that paragraph, that the photograph taken by Mr. G. H. Powell, Secretary of the Board of Ordnance and Fortification, and which he has reproduced as Plate 3, shows very clearly that the lower guy-post of the front wings has been bent back by its having hung on the launching car, and that through such hanging it has twisted the bowsprit and front nose of the framework around through an angle of twenty degrees or more, thus permitting these front wings to twist as described in the said paragraph. Mr. Brewer's statement that " it also will be observed that the rear wings are likewise distorted, though to a less degree," is a mis-statement of fact, as there is no distortion of these rear wings, either in Plate 3 or in Plate 4. In fact, there is nothing more noticeable on Plate 4 than that the rear wings are not in the least distorted, although they must have been supporting their portion of the weight at the time, while the front wings are distorted, as would naturally be expected with the front guy-post bent backwards to the position shown in Plate 3.

Mr. Brewer's statement is misleading in connection with his reference to the metal cap, which held the front guy-post from rising during the launching, having had to sustain the entire lift of the wings during such launching, since the metal cap would only have to sustain such lifting force of the wings as was in excess of the weight of the machine at that end, and as the launching speed was made to be the same as the soaring speed, there would not be any lifting force of the wings in excess of the weight except such as might come from a wind, and it was not expected that the machine would be launched at first in anything but a generally quiescent atmosphere, and there was no appreciable wind blowing when the machine was launched on October 7th, 1903.

It is also noted that among the other evidence in Mr. Brewer's possession, which he has failed to lay before you, is the statement which Dr. Langley made to the Press on the next day to correct the earlier one which I had given to them before having had an opportunity to examine the machine, or even to think quietly about the matter. Dr. Langley's statement was as follows:—

" Mr. Langley states that he was not an eye-witness of the experiment at Widewater yesterday, having been detained in Washington by business, but that on the report of Mr. Manly, immediately in charge, he is able to say that the latter's first impression that there had been defective balancing was corrected by a minuter examination, when the clutch, which held the aerodrome on the launching ways and which should have released it at the instant of the fall, was found to be injured.

" The machinery was working perfectly and giving every reason to anticipate a successful flight, when this accident (due wholly to the launching mechanism) drew the aerodrome abruptly downward at the moment of release and cast it into the water near the houseboat. The statement that the machine failed for lack of power to fly was wholly a mistaken one.

" The engine, the frame, and all the more important parts were practically uninjured. The engine is actually in good working order. The damage done was confined to the slighter portions, like the canvas wings and propellers, and these can be readily replaced.

" The belief of those charged with the experiment in the ultimate successful working of the machine is in no way affected by this accident, which is one of the large chapter of accidents that beset the initial stages of experiments so novel as

the present ones. It is chiefly unfortunate in coming at the end of the season when outdoor work of this sort is impossible.

"Whether the experiments will be continued this year or not has not yet been determined."

Mr. Brewer's supposition that I did not feel, or remember, until some years later, the shock experienced in launching is quite in keeping with the many inaccuracies of statement which he has made in the paper, as could be very easily demonstrated, were it desirable to do so, by the testimony of a number of those who were present at the time and with whom I discussed the matter while we were investigating the cause of the machine having pitched downward at such a sharp angle when it was launched.

Referring to Mr. Brewer's comments on the second launching of the Langley machine on December 8th, 1903. Mr. Brewer has not seen fit to even mention that, although the conditions at the moment of launching had become so bad, due to the fact that the houseboat, which could not be controlled by the tug-boat which was lashed to it, was veering rapidly under the combined influence of a strong tide and a strong counter wind, and that this combined with the lateness of the hour and the river being full of floating ice, made it most hazardous to attempt to launch the machine, still, owing to the depletion of the available funds and the feeling that it was "then or never," as explained fully in the Langley Memoir, I decided to take a chance. It was, no doubt, a grave error in judgment, but under the stress which existed at the time it was perhaps human, if not pardonable. What really occurred in the second test I have set out to the best of my ability in the Langley Memoir. It may be briefly summarised into the statement that with the boat swinging first in one direction and then in the other, as it was under the counter influence of the wind and tide, the machine was caught, while running down the launching track, by a wind gust striking it from a direction that it was not any more prepared to meet, or designed to meet, than many later and unquestionably successful machines, which have been wrecked by wind gusts on a flying field, and that the large tail rudder at the rear dropped down at its rear end to the track as the machine was running along it, and that this pulled the entire machine down into the launching car at the moment of launching so that both rear wings were destroyed along with the rudder, and the right hand front wing, and probably the front guy-post, were similarly damaged, as the bowsprit is seen very clearly in Plate 5 to be bent down somewhat as it was in Plate 3. What probably gave way first was the upper guy-post of the large rear rudder and tail. This guy-post, which was at the extreme rear of the main frame, does not happen to show in any of Mr. Brewer's plates, but is shown very clearly in Plate 53 of the Langley Memoir. It is there indicated by the numeral 43, being provided at its upper end with a pulley over which ran the upper control wire by which this tail was operated up and down for longitudinal balancing and steering vertically. This guy-post was braced by a horizontal wire 44 running from the top of it to the upper guy-post of the rear wings. As the swivel mounting of this large rudder had been clamped to the short vertical tube at the rear of the machine to prevent the rudder from turning in a horizontal direction during this first test, leaving it free to be operated up and down only, it was expected that the only pull which this rudder would exert on this rudder guy-post would be straight backwards and downwards, and therefore it was not braced except in this direction by this single wire just described. It is quite probable, therefore, that with the houseboat swinging around as it did, the clamping of the swivel fitting of the rudder did not hold it against swinging around under the influence of the wind striking it from the side, and that this caused the rudder to pull on this guy-post in a direction diagonal to this horizontal bracing wire at the top of it, and that the guy-post gave way under this diagonal pull, thus permitting the rear end of the rudder to drop to the track, which would readily account for all of the resultant pulling of the machine down into the

launching car when it was released and the smashing up of the rudder and rear wings and the damaging of the front wings, as seen in the photograph, Plate 5. I do not assert, however, that we know definitely that this is the way in which the accident started. I have stated very frankly in the Memoir that we did not know definitely exactly how the accident started, or the sequence of events, except that the large tail rudder was seen by Mr. Reed to be dragging on the track at least ten feet or more before the machine was launched, and that the smashing up of this tail rudder by this dragging of it caused the whole machine to be pulled down into the launching car when it was released, resulting in the damage already described. Mr. Brewer, who was not present, maintains that he knows all about it, and that it was caused by weakness of the wing structure, but none of the score or more of those who actually witnessed the accident were able to give any description of just what occurred first earlier than their seeing the tail rudder dragging on the track, though all who were able to give any detailed description of what they saw concurred in stating that the tail rudder was dragging on the track before anything else appeared to go wrong.

Regardless, however, of whether or not the accident in launching the machine on December 8th, 1903, was due to a side wind striking it, the main point is that the machine, both in design and construction, was sufficiently strong to safely transport itself and its operator under the quiescent atmospheric conditions for which it was designed and under which the first tests were supposed to be made. The rear wings and the tail rudder with its operating cables and supporting guy-posts had shown no weakness in the first test on the 7th of October when the tail rudder was subjected to even heavier stresses than it would normally be called upon to withstand, due to my having pulled it up to the upper limit of its motion, or negative angle, in trying to right the machine when I found it plunging nose downward in this test.

There is no foundation for Mr. Brewer's statement that "there were several perfectly good reasons why the wings would collapse under any conditions of the launching." He cites as one of these reasons an alleged weakness of the cross ribs of the wings as shown in the sand loading tests, for which the deflection figures are given in the Memoir, and states that "when a sand load of only twenty per cent. above the flying stress was imposed, most of the ribs were bent from twelve to thirteen inches out of shape." He does not in any way distinguish the difference between flexibility and weakness. Dr. Langley's method of development was not to guess at anything, or depend on mathematical computation, where exact data could be procured by tests. It was for this reason that he devised, many years before I was associated with him, the scheme of loading the wings with sand to predetermine not only whether they were strong enough to stand the loads which it was known they would have to meet, but also to determine whether the change in contour was sufficient to interfere seriously with their effectiveness as supporting surfaces. Dr. Langley insisted most strongly at all times on the wing structure being kept as flexible as possible without such flexibility militating too greatly against their effectiveness. His opinion was that if the wings were too rigid in the early experiments, it would result in the machine being much less stable, and his great concern at all times was to minimise, as far as possible, the danger of a fatal accident occurring in the early trials before the operator had become sufficiently experienced to be able to manage the machine effectively. His greatest concern at all times was longitudinal stability, and he felt that this must be automatically maintained, certainly until the operator became quite experienced in controlling the machine. He insisted, therefore, that the wings for the large machine, especially at their trailing edges, should have practically the same degree of flexibility in proportion to their size as the wings of the successful models had. The cross-ribs, while, therefore, flexible to the extent shown by the deflection figures in the sand load tests, were not weak, but even stronger in proportion to the load they had to carry than those of the successful

models which had not only shown no weakness in flight, but had not even been broken in many of their landings on the water. The wings on the quarter-size model, which flew on August 8th, 1903, were proportioned in all their parts with reference to the corresponding parts of the large machine in exact accordance with the relative weights of the two machines, and there was certainly no weakness apparent in this quarter-size machine. A number of very clear photographs of it in flight are given in the Memoir.

In discussing the position of the guy-posts and the centre of pressure on the Langley machine, Mr. Brewer makes the statement, "Mr. Langley had made no measurements to locate the centre of pressure at small angles." He thus asserts as a fact what is untrue. For several years before I became associated with Dr. Langley he had been making a very thorough study, by actual tests, on the whirling table of the position of the centre of pressure of a score or more different shapes of curves supporting surfaces, a large number of these being indicated by the letter "T" in Plate 48 of the Memoir, where they are shown hanging on the wall in the background of the picture. Furthermore, in the Memoir, in describing the supporting surfaces, I have stated that the main rib (or wing beam) was placed at approximately the point where the centre of pressure of the wings was at their flight angle of ten degrees, and that this main rib was located at about forty per cent. from the leading edge of the wing. The reason the guy-posts were located as they were on the large machine was solely and only because they had been located at this point on the small machines, and that shop tests of both machines had shown that the system of guying was thoroughly strong and effective.

Mr. Brewer has made great point of the fact that the camber of the wings of the Langley machine in the Hammondsport test was not the same as that of the wings in the tests of 1903. I have clearly stated in the Memoir that the wings for the large machine were originally made with a one in eighteen camber, and that later when the rib construction was improved and the wings, which were finally used in 1903, were made in 1900, the front extension or leading edge was added, which changed the camber to one in twelve, to make these wings more nearly a duplicate of the wings of the steam-driven model No. 5 which had flown in May, 1896, instead of like those of No. 6 which had flown on November 28th, 1896, as they were originally planned to be. Both of these wing curves had given very good results on the whirling table tests, and Dr. Langley favoured the one in twelve camber and ordered it used in the final wings. It is quite certain that, aside from the greater cost of building the wings with this front extension, there would have been no particular difference in the results of the Hammondsport tests had they been so built. I am quite certain that any unprejudiced observer, who could have seen the Langley machine as it was rigged up at Hammondsport with the heavy floats attached to it and with the heavy, cumbersome system of rough struts and braces used in attaching it to the pontoons, would not have thought that it stood as good a chance of making a successful flight, using the crudely constructed wings with which it was equipped, as it would in exactly the condition obtaining in the tests in 1903, or as it now is in the National Museum in Washington.

Owing to the very brief time available between the receipt of the advance copy of Mr. Brewer's paper and the close of the last mail which will enable these comments on it to reach London by October 20th, it is impossible to attempt to go into Mr. Brewer's paper in more detail. I will only add, as I have always stated and do affirm, that the accident in the test of October 7th, 1903, was due entirely to the hook on the end of the lower front guy-post catching in the launching car, and that but for this accident a successful flight would, no doubt, have been achieved on October 7th, 1903. I have always and do still maintain that the accident to the machine on December 8th, 1903, was due to its being caught in a squally wind, which, under the influence of the counter force of the tide and wind,

caused the houseboat to swing around in such a way that the machine was subjected to stresses which it was not expected that it would be subjected to, nor necessary that it be subjected to, in order to give it a test in free flight, and that the machine, exactly as it existed in December, 1903, was thoroughly capable of making a successful flight and demonstrating that it was the first machine constructed in the history of the world capable of successfully transporting a human being through the air and being properly controlled in such flight. The tests at Hammondsport certainly demonstrated that the machine was capable of doing more than had ever been expected of it in the matter of carrying weight, and that the original engine with which it was equipped not only furnished sufficient power to enable it to carry its original weight, but also a greatly increased weight requiring considerably more than fifty per cent. more power than the original weight required, and that the claim is thoroughly well founded that it was the first machine built capable of safely transporting a human being through the air and being properly controlled in such flight.

From Mr. GLENN H. CURTISS.

Garden City,
New York.

THE TRIALS OF THE LANGLEY FLYING MACHINE AT HAMMONDSPORT, N.Y.

I have read the proof of Mr. Brewer's Paper on the Langley Flying Machine. In the first paragraph Mr. Brewer quotes Lord Northcliffe as saying that " in the United States there have been long and persistent attempts to belittle the work of Orville and Wilbur Wright." I think Lord Northcliffe has been misinformed. Personally, I have always thought the Wrights are entitled to and have received full credit for having invented and built the first airplane to make successful flights. The Langley Flying Machine of 1903 *did* however fly in 1914 at Hammondsport, N.Y., in its original condition with its original motor and propellers with no alterations except the addition of floats and their necessary supports, weighing altogether 350lbs. On several occasions this original machine rose from the water and flew for a short distance, and there is at least one photograph of the machine in the air.

Some time prior to June, 1914, I received a letter from Dr. Walcott, of the Smithsonian Institute, stating that an appropriation had been made by the Trustees of the Institute for the expenses of having the original Langley Flying Machine of 1903, which had been wrecked in launching at that time, given a trial flight and asking if I cared to undertake the commission. As we had every facility for doing the work and were very much interested in learning what the Langley machine would do, I replied that I would accept the commission. The machine was shipped to Hammondsport and assembled for the trial. We fitted three pontoons, two forward and one aft. The two forward pontoons were placed in such a position under the forward wing so that the angle of the guy wires leading from the wings would be unchanged. It was, of course, necessary to remove the long post to which the lift wires had been attached when the machine was launched from the catapault. I personally made the early trials and succeeded several times in rising from the water and flying for a short distance, carrying the additional head resistance and weight of the pontoons and fittings, which latter we found was about 350lbs. The dihedral wings gave excellent stability, and we were so pleased with the fact that we were able to fly the machine with this great amount of extra weight that we asked permission to instal a more powerful motor with a direct drive propeller and make more extended flights. This installation was made and flights were then made with the machine by two of my assistants,

The question of whether Dr. Walcott is prepared to concede that the Wrights were the first to construct a machine capable of sustained free flight does not concern the subject of my paper, because the decision on this point is not submitted to him for judgment. The claim of originating the first aeroplane capable of sustained free flight is put forward by Dr. Walcott after Mr. Langley's death as being due to his predecessor in the Smithsonian Institution, and he cannot pronounce judgment on this question and plead his claim at the same time.

Dr. Walcott touches the root of the argument when he claims these hops were "flights," because the official observer whom he appointed acknowledges that they were not really flights, because there was not sufficient propeller thrust. Flying may be compared to a person swimming. A person can either swim or he can't swim, and it is no use discussing how nearly he swims if he can't keep his head above water. An aeroplane can either fly or it can't fly, and it is no use discussing how nearly it flies if it has not enough propeller thrust to enable it to stay in the air as long as that amount of thrust lasts. The observer appointed by Dr. Walcott has stated that it was necessary to increase the propeller thrust. A Curtiss engine and a Curtiss propeller were therefore installed. The purpose of the propeller thrust was two-fold, *i.e.*, (1) to propel the Hammondsport machine on the water, and (2) to make it fly. We are all agreed that the machine had sufficient propeller thrust to propel it on the water, so it was not necessary to increase the power for this purpose. It was, however, according to Dr. Zahm, necessary to increase the propeller thrust, and to make the Hammondsport machine fly was the only other object. It is therefore obvious that the increased thrust, which Dr. Zahm says was necessary, was to secure sustained flight, which had not been attained. Even when the power was nearly doubled by substituting the Curtiss engine and modern propeller for the old Langley engine and twin propellers it could not make the machine fly, but only enabled it to leave the water when headed into the wind between the lulls and assisted intermittently by the gusts of wind.

In reply to Dr. Zahm, we all appreciate that unnecessary personalities, innuendos, and imputations of bad faith should be avoided so far as it is possible to avoid them, but nothing can be gained by indirect language.

It is in starting from an unsound hypothesis that Dr. Zahm arrives at erroneous conclusions. His simile of the eagle and the baby is inapplicable to the Langley machine, because the eagle had flown before losing its feathers. The bird he must choose to correct the simile is the ostrich. Even a scientist of the type referred to by Dr. Zahm cannot prove that an ostrich will fly by merely pulling out some of its feathers, loading it with misplaced weight, and then assuming that the handicap imposed warrants the assumption that it would have flown if the poor bird had been left alone to begin with.

Dr. Zahm now acknowledges a change of the aspect of the wings. He says, "True, the overall aspect ratio had been increased slightly; but what was the gain in efficiency—three or four per cent.?" Dr. Zahm further acknowledges that the "arched leading edge" was omitted at Hammondsport. A change in camber from one in twelve to one in eighteen made by these alterations improves an aeroplane surface of the Langley type by over twenty-five per cent. How can Dr. Zahm's acknowledgment now of these changes be reconciled with his statement that "the wings were identical in construction with the original machine, except, as I have heretofore stated, they were perhaps a little more roughly built and a little heavier."

Dr. Zahm now further acknowledges that the Hammondsport trials proved nothing as to the structural strength of the Langley machine. How does he reconcile his statement now with his previous statement in the Smithsonian Report which I have already quoted in my paper on page 630, in which he pretends that

the Langley trussing designed to carry 830 pounds carried 1,520 pounds actual weight in the later tests?

Dr. Zahm is aware that his statement is untrue, and when his attention is drawn to it he is silent. It would be idle to further discuss Dr. Zahm's statement while these glaring discrepancies exist between his statements and the facts.

Coming to Mr. Manly's lengthy statement, I approach this with some diffidence, because of my admiration for his courage in attempting to pilot the original Langley machine in 1903 at imminent risk of his life. For a man with no flying experience, such as the later pilots of the Hammondsport machine had acquired after the Wrights built the first man-carrying aeroplane, he was bold beyond conception, especially so when he knew that the machine he was to test had so small a chance to fly.

Mr. Manly reiterates the reasons on which he based his belief that the failure of the Langley machine in 1903 was due to an accident to the launching gear. My review of the same evidence I contend clearly proves that Langley failed because his machine was not capable of flight. It is for independent readers of the AERONAUTICAL JOURNAL to judge between us, and I venture to predict that the attempted flights of the Hammondsport machine in 1914 will not strengthen the claim of the Smithsonian Institution.

Mr. Glenn H. Curtiss is content to reiterate that the Langley flying machine of 1903 did fly in 1914 at Hammondsport "in its original condition with its original motor and propellers with no alterations except the addition of floats and their necessary supports." I was not content in my paper to merely deny this statement which Mr. Curtiss has made many times before, but I produced photographs and other proof to show that the Hammondsport machine was not the Langley machine with merely these differences, and I also submitted proof that even with this amount of changing the machine did not fly. Mr. Curtiss's flat contradiction of the photographs given in my paper carries no weight, especially as Dr. Walcott, Dr. Zahm and Mr. Manly acknowledge the changes and do not support him in this contradiction.

A member of the Society has suggested that my paper related to a purely commercial dispute on patent rights which could be more suitably raised in a Law Court. A Court of Justice could not be invoked by me to judge whether the disclosures in my paper are true or false. Only at the instance of Dr. Walcott, Dr. Zahm, or Mr. Curtiss could the truth of these disclosures be judged in a Court. My paper has nothing to do with any patent rights, nor to any commercial consideration. I had no financial interest to serve in writing my paper, and I have no connection, and never have had any, with the American Company which owns the American Wright patent.

To discuss the statements and conclusions given in such minute detail in these four letters would divert the issue from the real facts. The main purpose of my paper is to explode the fable that the Langley machine had been flown at Hammondsport. Neither Dr. Walcott, Dr. Zahm, nor Mr. Manly deny any of the changes pointed out in my paper. Mr. Curtis alone denies them, but he offers no proof to refute the photographs showing the changes he had made before the first trials. Dr. Walcott, Dr. Zahm, and Mr. Manly are content to contend that the changes were all made for some other purpose, which, far from helping the Hammondsport machine to fly, acted rather as a handicap. History does not want assumption and deductions. When a fact goes down to posterity, it must be based on something that has been done, and even those associated with the Smithsonian Institution cannot be credited with deeds accomplished merely because of their high scientific standing and their belief that these deeds could have been done. By all means let Dr. Walcott, Dr. Zahm and Mr. Manly believe that Langley's machine

would have flown if merely fitted with floats, handicapped with added weight, and with a skilled aviator in Mr. Manly's place. But they must stop there, and not say that the Langley machine has been flown, merely on the strength of their alleged belief that it could fly. No, the Langley machine is just where Langley left it—a beautifully executed delicate work suitable for a museum, but impossible in the air. It has never been flown and it never will fly.

[EDITORIAL NOTE.—*Correspondence is invited on the above lecture and discussion.*]

THE MANŒUVRES OF GETTING OFF AND LANDING.*

Lecture delivered before the Society on November 3rd, 1921, by Squadron Leader R. M. Hill, M.C., A.F.C.

DISCUSSION.

Major-General Sir SEFTON BRANCKER said he knew Squadron-Leader Hill as one of our most daring, cool-headed and really scientific pilots. He had also realised that evening that he had a brilliant fund of imagination as well. His (Sir Sefton Brancker's) chief qualification for talking about getting off and landing was that he was once more notorious for making bad landings without hurting himself or anyone else than anyone in the Air Force. He was all for eliminating the human factor in control as time went on. He thought the devices which Major Hill had explained were absolutely on the right lines of progress. Pilots did not like the idea, but he was all for developing the stabilisers, height indicators, and all the gadgets that the Author had described. But there were limits to this policy. Some time ago there was an official objection from the French at Le Bourget that the British pilots were stunting. The stunt in question was the ordinary side-slip landing. He claimed that this was a perfectly legitimate manœuvre, and was probably the best form of landing; it should certainly not be counted as a stunt. From the commercial point of view, apart from safety, landings were of enormous importance, because heavily-loaded aircraft, if badly landed, received a serious shock throughout their structure which threw them out of truth and caused constant trouble, and extra labour, and more rapid deterioration, whereas with a good pilot who put them down very gently, they lasted longer, there was far less labour to be expended on keeping them in truth, and the depreciation involved was not nearly so great; so much so that he fancied the Oleo landing gear, or some equivalent, would have to come back for commercial machines; indeed, he thought that probably all the latest types had them. The speaker had mentioned one drawback to the Oleo gear in that part of his paper which he had not read that evening; it was not so good for getting off as the rigid type, which bumped a little and helped the machine into the air. The Oleo did not bump at all, and so full flying speed was necessary before one could lift the machine from the ground.

Captain BARNWELL said he had prepared a few notes on the written paper, but as they all referred to bits of the paper which had not been read, the ground was rather cut from under his feet. There was one point to which he would like to call the Author's attention:—He first of all told them that an aerodrome was the "pilot's paradise," and later in the lecture mentioned that the average aerodrome bumped a machine in taking off. The two statements hardly seemed compatible. He rather agreed with the latter description. It was impossible for him to talk with any authority about getting off or landing, because they were both parts of flying that needed a great deal of practice and observation before one felt competent to decide what one actually did and did not do, and in the very nature of things one gave the least possible practice to them, because they were the occasions most dangerous to the welfare of the aeroplane. He was strongly of opinion that more was to be gained by improvements in landing gears than by attempting, by some variable property of the wings, to reduce landing speed. His own feeling was that it was possibly easier to get into a field at, say, 100 miles an hour than at, say, 50, particularly if it were gusty and bumpy, and he thought one would come into the field at the 100 miles an hour quite

* See page 510.

happily if one had an under-carriage that would stand the greater vertical velocity (assuming vertical velocity as a function of horizontal) and had efficient braking devices which would stop the aeroplane safely in a short distance.

(*Communicated*): I cannot entirely agree with Major Hill that the proximity of the ground influences the pilot's handling of the aeroplane in taking off from an aerodrome. Personally, I believe that, almost directly after I have opened the engine "full out" (for taking off), I look practically at the horizon, and the distance the machine is above the ground does not consciously affect me.

Again, I am not certain that, in getting off, one finds any serious disadvantage in holding the machine on to ground up to a speed considerably above stalling, except perhaps for fear of breaking the propeller.

Regarding Major Hill's statement as to the behaviour of normal single-engined compared to that of normal twin-engined machines with respect to balance when "running up" engines on the ground, I imagine that slipstream effect on the tail plane should be as great for the normal twin as for the single, and am inclined to believe that the "nosing-over" tendency of the twin (noted by Major Hill) is almost entirely due to the lower chassis (relatively) of the twin.

Major Hill has not remarked that the "turning" tendency of the single-screwed aeroplane is due to the rotary motion of the slipstream; this "turning" tendency is of the sense noted by Major Hill, because the fin surface is mainly, or entirely, in the upper half of the slipstream; could one place the fin surface right on the centre of the slipstream there would be practically no "turning" tendency; could one place it mainly (or entirely) in the lower half of the slipstream the "turning" tendency would change in sign. In this connection one would very much like to know what "rudder" is necessary with the Parnall "Puffin," in which the fin is below the fuselage, and with, say, the latest Dornier flying boat, in which the airscrew axis appears to lie near the top of the fin.

Another point in connection with the "turning" tendency of single-screw aeroplanes:—The engine torque must tend to "bank" the machine, and a machine when "banked" tends to turn in the direction of the lowered wing tip if (as is usual) the "centre of pressure" on the total vertical fin surfaces lies aft of the C.B. It is probable that this "turning" tendency (due to torque) is very small, but it would be of interest to know of what order of magnitude it is compared to that due to slipstream on fins. It would also be very interesting to try whether, by placing suitable fin surfaces close behind the airscrew, one might not practically eradicate the rotary velocity of the slipstream and thereby eradicate any "turning" tendency.

A final point about "turning" tendency:—I have flown at least one machine of which I have gathered the impression that the "turning" tendency on the ground was of opposite sign to that in the air; that is to say, the machine (an R.H. tractor) tended to turn to the right taxi-ing, and to the left (*i.e.*, normally) in the air.

I have imagined that possibly "bending" of the slipstream, due to the ground surface, may account for the phenomenon; I should like to know if this "reversal" has been noted by anyone else, and if so, what is the reason for it.

I entirely agree with Major Hill that a low undercarriage, meaning only a small rake of the fuselage on the ground, is on the whole preferable for getting off, for taxi-ing, and for landing; the only disadvantage is the generally accompanying small angle of attack of the wings; but I feel quite certain that eventually we shall achieve ground brakes much more efficient and satisfactory than wing-drag. It should be noted that high wing clearance and high chassis, and (still more) small wing clearance and low chassis, are not necessarily synonymous.

Except for the possibility of damage to the wing itself, I am sure that the nearer the bottom wing is to the ground, the better for landing; but it would

appear that if a wing is higher than, say, one-half its own chord length above the ground surface there is little practical advantage from the proximity, so the larger the machine the more this advantage may be realised.

Major Hill mentions "bouncing" quite frequently. I feel sure that this is largely a temporary evil due to the somewhat primitive form of undercarriage from which it was impolitic to depart during the war; so although bouncing, with its accompanying effects on get-off and landing, is at present a serious consideration, it will not remain so much longer.

Major Hill would appear to include (and that necessarily) among the normal possible sources of danger in getting off and landing factors which might rightly be attributed to bad aerodynamic design. I grant that most, if not all, present-day aeroplanes are more or less badly designed; but it should not be long before we design machines without, say, appreciable yawing moment due to lateral control and without the possibility of any one control limiting seriously the power of any other.

I should like to corroborate, from unpleasant personal experience, the great difference between landing the average aeroplane (with a fairly high-powered stationary engine) throttled "right down" and "switched off" (or with the engine otherwise stopped). Such landings as most of us have the opportunity of practising are all "throttled," and I am perfectly certain that, to be as prepared as possible for a forced landing, the pilot must practise landing between marks, say, with his engine switched off.

I would also suggest that in the testing of a new design, landing switched off should always be included—it is quite possible that it might lead to modifications being found advisable.

I should like to express my personal indebtedness to Major Hill for the interest and instruction I have obtained by reading and thinking over his paper.

Wing-Commander CHRISTIE said the Paper had explained many phenomena which had hitherto been mysteries—to him, at any rate. Many heavy-handed pilots like himself with a great number of "write-offs" to their debit or credit would do well to digest the valuable tips the Author had given. He would limit himself to one or two points. The influence of wind on starting and landing manœuvres had been discussed, but, in addition, the changeable character of the atmosphere appeared to him to add to the pilot's difficulties considerably. It was his experience, and also that of others, that one had to land at a considerably greater speed in a warm damp temperature than in a dry air of the normal temperature to avoid stalling. Taking a high temperature as 30°C., or 84°F., and the air to be saturated with moisture, there was a decrease of approximately 7 per cent. on the specific density of the atmosphere as against the normal conditions at home at 10°C. That small advance did not seem to offer an adequate explanation. Could the increased amount of the water vapour in any other manner affect the lifting ability of the planes?

The Author's remarks on air brakes were interesting. It seemed probable that if an efficient air brake could be designed, its use in machines landing by night would be considerable, as it would permit a much steeper glide and lessen the danger of aircraft striking obstacles on the approach to the flare-path.

Regarding the Palethorpe tail-skid, how did this assist one to land in the dark? He knew remarkable landings were made by Captain Palethorpe at Andover, but he could not picture the functions of the tail-skid from the short description in the paper. The Noakes ground indicator seemed to be very much in the right direction, but did it punish a fast landing and could it be used on an aerodrome surface that was rough or uneven? Also was it effective in landing against strong winds? He suggested that the Author's organisation of an air port of the future was technically possible to-day, only the traffic and finances were lacking to support it.

Mr. GREEN said he was most interested in the landing gear question. He considered and he thought that Captain Barnwell would agree with him that under the stress of war conditions designers had provided bad landing gears, and it had only been due to the skill of the people using them that crashes were not more frequent than they have been. The Lecturer showed a picture of an aeroplane on its back, fitted with a landing gear which he (Mr. Green) had the privilege of designing, and of which he was now rather ashamed. It had been used during the testing of an automatic landing gear, and he (Mr. Green) felt confident that for the same weight and resistance as that landing gear a different gear could be now made which would have prevented the accident.

He thought that in the future improvement in landing gears would be made which would call for less skill on the part of the pilots.

The Author suggested that the Oleo gear was worse for getting off, but it seemed likely that the difference was so minute that another five revolutions of the engine would cover it. Squadron-Leader Hill thought that landing gears with greater shock absorption would be a great advantage. Landing gears had already been made of much the same weight as we formerly used, but with three times the shock absorption. It would not involve much weight to design landing gears to give six times the shock absorption, but it seemed hardly necessary to allow for more shock absorption than was sufficient to absorb shock of landing at the normal gliding angle of the machine.

There was one manœuvre on landing which on most machines was impossible. When one reached the ground one frequently wanted to stop quickly. This could be done by brakes or by swinging the machine round quickly. It was the latter manœuvre which could not be done on the average aeroplane to-day as one got a wing tip in the ground and turned over. Landing gears could be made which would admit of rapid turning on the ground, and he did not think that the increase of weight would be serious. Of course, the landing gear would need to be made of wider track and stronger sideways. Looking into the future, he thought they would see aeroplanes landing fairly fast and swinging round in a very small circle. With a hydraulic landing gear one might even turn at high speed on the ground by causing the Oleo leg to bank the machine in such a way that the wings could take the lateral force and stop the aeroplane skidding sideways.

Wing-Commander BOWEN said so far as his own landings and getting off were concerned, they made him think the age of miracles was not yet past. He was interested chiefly now in the instrument side, and one felt that there were extraordinary difficulties in the instrument side. The branch of research which he was in had been trying proximeters, *i.e.*, electrical devices for telling a pilot when he was getting near the ground, and so on. They realised the enormous difficulty of it, but had not done much else. He thought there must be some mechanical solution of the problem. The two they had seen that evening seemed very helpful. Wing-Commander Christie made an interesting point about landing in mists. In the East landing was complicated by higher landing speeds. Had the Author anything to say on how to compete with conditions in some parts of the East where the ground wind might be ten miles an hour and boxing the compass in three minutes or less?

Captain G. T. R. HILL said it was nine years ago that day since his brother and he put a full-sized glider into the air. That was, he thought, the beginning of the Lecturer's active study of getting off and landing, and of the effect on the structure of the machine of contact with the ground. Since that time the Lecturer had made a large number of landings, some good, some bad, and he thought that the results, analysed and collected as they had been, formed a valuable contribution to the knowledge of all, particularly those who were not pilots themselves.

To give an example, the other day a so-called deck-landing expert gave forth as his considered view that aeroplanes for deck landing were all right and the

ships on whose decks landings were to be made were all right, but all that was required to achieve greater success was more confidence on the part of the pilots. He thought he was absolutely wrong. The aeroplanes were wrong and the ships were wrong. That did not mean that they were not good, but that they might be better. He was not prepared to throw the whole blame of any lack of success there might be on the pilots. He hoped this paper would enlighten those who did not fly on the difficulties and trials the pilots had to face. On the other hand, after a long experience of flying it was difficult to put oneself in the place of someone learning and feel again the old difficulties and trials.

A technical point upon which he would like opinions was the minimum clearance necessary between the lower plane and the ground. It was generally agreed that it was easier to land with the lower wing close to the ground. On the Vickers-Vimy which had been shown, the bottom plane was about 3ft. from the ground, and the same figure held roughly for the little Bristol. On the large aeroplane that distance, relative to the chord of the wing, was much less. It would be interesting to have various pilots' opinions as to whether the plane should be placed a certain absolute distance from the ground regardless of the size of the aeroplanes, or whether it should depend on the span or chord of the wing. On the one hand it might be argued that machines have to land on the same sort of aerodrome, and the minimum height necessary should be considered to be that which would just clear small bushes or obstacles, but on the other hand, when landing with a small amount of bank the height of the lower wing must depend to a certain extent on the span of the aeroplane if the wing were not to touch the ground.

Captain Barnwell's statement about landing at 100 miles an hour sounded rather dangerous, and he thought the insurance companies would take that view. It was all very well to excuse these high landing speeds by good undercarriages, but one wanted a comparatively slow landing and a good undercarriage too.

He would confess to being responsible for one of the crashes shown on the screen; the picture was, however, used to illustrate the correct way of crashing without hurting oneself. He hoped any pilot who found himself just about to crash would not review his past life in that last moment, which he is usually supposed to do, but would remember the picture which had been shown and crash in the correct manner.

In reference to a question by Mr. McKinnon Wood as to how he did it, Captain Hill said it was difficult to give a very clear account of what happened. The main idea was to put one wing down so that, being below the level of the undercarriage, it would touch the ground first. He did not think that there was anything in bringing a wing forward by using the rudder.

Mr. McKinnon Wood said he could not speak as an expert as he had never done any of these manœuvres. He was interested to know on what lines research and design ought to be developed. He also was surprised to hear Captain Barnwell say landings should be made in the future at 100 miles an hour. He had always understood low landing speeds were desirable, and he felt that was so, coupled with a very improved undercarriage. The subject of landing had been neglected, because we had passed through a war period when one was forced and prepared to take much greater risks. He thought it would be agreed that there were much too many crashes in landing. The high landing speed might be all right in a good aerodrome, which might have larger dimensions than at present; but even if effective brakes were provided the pilot would not be in a happy position when he had to execute a forced landing.

Flight-Lieutenant Noakes, who was responsible for the ground indicator and who was asked by the Chairman to answer Wing-Commander Christie's questions, said it was purely an idea for landing at night or in fog. The device had a Vernier adjustment. If one came in to land at 60 m.p.h. the pull required on the

control column was more than required if landing at 70 m.p.h. There were oscillations when approaching the ground fast. The ground indicator worked just the same in a high wind. The ground indicator was attached to the control column by a wire and shock absorber so that the pilot could overcome all the pull which was exerted on the control column by the indicator arm being on the ground. For each type of machine the correct pull required on the control column would have to be found out by a series of flights.

Squadron-Leader HILL, replying to the remainder of the discussion, apologised for not alluding to the paper in greater detail. He had been afraid of taking up the time unduly, so he had tried to convey the spirit of the paper rather than parts of the letter. The point referred to by Sir Sefton Brancker, of what were called stunt landings, was certainly a vexed one. The sideslip landing was hardly a stunt, it was a gentle gliding manœuvre; in fact, to those who were not accustomed to watching aeroplanes fly, it would hardly be apparent that the aeroplane was doing a sideslip landing. One would notice that it was canted over on the glide down, but not that any violent contortions or evolutions were being carried out. The point about consistently good landings was an interesting one. If any aeroplane were consistently badly landed one found, after a bit, that the various fittings crushed into the wood a little—enough to make one feel unhappy—and trouble of that sort made the effective life of the aeroplane much shorter than it should be. He must yield to Captain Barnwell the point that his (the Author's) remark about an aerodrome being a pilot's paradise was inconsistent with the later statement about the bumps. He thought that improvement in undercarriages was wanted, that reduction of landing speed was wanted, and that these, combined with the extra confidence that pilots would thereby get, would make civil aviation a really sound proposition. In regard to Wing-Commander Christie's remarks on the influence of wind, it was an observed fact that if one landed in a strong wind one felt the aeroplane dropping unexpectedly. The question as to what happened in a warm damp temperature he would like to leave to someone more competent to answer. He had not observed very much there. Anything new that was observed would be very valuable, and that was what one hoped would come out in a discussion of that sort. He would not like to say anything about moisture. The effect of heat in the East they did, of course, know about, and, apart from the height of some aerodromes which increased the landing speed of aeroplanes, there did seem to be an effect due to the temperature itself. Wing-Commander Christie's other questions, he thought, had been answered by Flight-Lieutenant Noakes. The use of the ground indicator, as Flight-Lieutenant Noakes described it, was perfectly possible on the S.E. 5A. If one decided to glide down at 70 m.p.h., one could glide down to the ground with one's head near to but not actually touching the control stick. When the ground indicator touched the ground it flattened the aeroplane out. If every time the aeroplane was flattened out in this way one just put the control stick forward again, one could go on undulating a few feet from the ground allowing the indicator to keep on touching it. Every time the indicator touched the ground the control stick would come back. With a little improvement the ground indicator would give the pilot all the confidence that was necessary. He was interested in Wing-Commander Bowen's statements as to the instruments he had been trying. Something was wanted to convey to the pilot vividly the fact that he was meeting the ground. He was like a man waiting for a bull to charge him and he was not going to look at small indications of needles or anything like that.

He (the Author) knew that Major Green had made an Oleo undercarriage for a scout which weighed no more than an ordinary elastic sprung scout undercarriage. That, he considered, was a real achievement. The Oleo undercarriage would have to come. A wide track would be necessary, whether it was a question of allowing one to swing round quickly without tipping over or simply to steady the aeroplane when making contact with the ground. Undoubtedly, improve-

ments would come. They would, he thought, come rapidly; and, as a commercial vehicle, they need not have any fear about the ultimate perfection of the aeroplane.

Wing-Commander Christie desired to know more precisely the function of the Palethorpe tail skid, especially in its capacity to assist landings made at night. Captain Palethorpe's idea was to glide down to the ground at a flat angle by using a certain amount of engine. When he thought he was a few feet off the ground he partially flattened out, but did not, as in the normal way, carry out the whole flattening out operation. The wheels would then strike the ground, perhaps rather hard, so that in the normal way the aeroplane would have bounced off the ground violently. As soon, however, as the aeroplane started to bounce the nose would go up and the tail come down, thus bringing the long tail skid into contact with the ground. The tail skid would absorb the shock by means of its Oleo. but at the same time stop the nose of the aeroplane going up further so that it would drop. The wheels would again touch the ground and bounce less violently. In practice this bouncing was damped by the Oleo tail skid almost at once. The net effect was to allow the pilot to flatten out only partially, instead of having to carry the flattening out operation to its conclusion, which is a more difficult thing to do correctly at night, and involves the risk of stalling too high above the ground.

Wing-Commander Bowen asked what the Author had to say about landing in the East when the wind boxes the compass in three minutes. If the wind changes so rapidly it may be practically impossible to land into it. The pilot is then forced to land across it. He must, therefore, be provided with improved undercarriages. This is where the wide track undercarriage will undoubtedly be of enormous value to him.

The CHAIRMAN, in moving a vote of thanks to the Author, said no doubt the moments of getting off and alighting were the moments of compulsory skilfulness in flying, and any instrumental assistance at that critical time might eventually be of service. He thought it was very interesting to hear the change of attitude that existed in this year of grace, compared to what it was eight or nine years ago or early in the war, when the attitude of many pilots towards any device that did what the pilot did previously was disliked, much as the workman disliked the spinning jenny because it was doing his job. Various devices were made, such as the now universal speed indicator, and the opposition to the scientific side really was, in his opinion and he thought in that of many others, a brake on the progress of aeronautics—a brake now happily removed. Such devices as had been described would build up the future of civil aviation and such a lecture as Squadron-Leader Hill's advanced the cause. It was an advantage to the Society to have Squadron-Leader Hill as lecturer, and it was also pleasant to know that the Research Committee had considered the Royal Aëronautical Society a good mouthpiece—which he thought it undoubtedly was—for approaching the public, the flyers, and designers for a discussion on this subject.

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